Three Essays on the Foundations of Public Policy Making

Dissertation

zur Erlangung des akademischen Grades
doctor rerum politicarum
(Doktor der Wirtschaftswissenschaft)

eingereicht an der
Wirtschaftswissenschaftlichen Fakultät
Humboldt-Universität zu Berlin

von
Lukas Mergele M.Sc.

Präsidentin der Humboldt-Universität zu Berlin:
Prof. Dr.-Ing. Dr. Sabine Kunst

Dekan der Wirtschaftswissenschaftlichen Fakultät:
Prof. Dr. Daniel Klapper

Gutachter:
1. Prof. Dr. Alexandra Spitz-Oener
2. Prof. Rajshri Jayaraman, Ph.D.

Tag des Kolloquiums: 13. Februar 2019

Abstract

Global indicators of democracy and civil liberties have continually decreased over the past twelve years. Trust in democracy as a form of government is at a low. With democracy being an important driver of economic development, this trend also stirs concerns about future economic prosperity. Scholars have identified weak public policy-making as an origin for low levels of trust in democratic governance. In three essays, this dissertation studies two reform options to improve policy-making, namely decentralization and privatization.

The first essay examines whether the decentralization of public employment services (PES) increases job placements among the unemployed. Decentralizing PES has been a widely applied reform used by governments aiming to enhance their efficacy. However, economic theory is ambiguous about its effects, and empirical evidence has been scarce. Using a difference-in-differences design, I exploit unique within-country variation in decentralization provided by the partial devolution of German job centers in 2012. I find that decentralization reduces job placements by approximately 10%. Decentralized providers expand the use of public job creation schemes which diminish job seekers' reemployment prospects but shift costs to higher levels of government.

Essay two explores fiscal interactions in Colombia, a developing country which shifted the responsibility for a large share of public spending from the central to local governments. I analyze whether public expenditures in neighboring municipalities influence local spending decisions of Colombian mayors. I offer a quasi-experimental identification strategy exploiting exogenous variation in municipalities' exposure to changes in the world market price of oil, depending on the municipalities' endowment with oil resources, and controlling for municipality fixed effects. I find evidence of strong spatial autocorrelation of local public spending. However, the quasi-experimental instrumental variable approach reveals that there are no significant causal fiscal interaction effects between municipalities. This highlights the importance of using additional sources of exogenous variation for the identification of fiscal interactions. In the developing country context, our findings suggest that fiscal decentralization policies in Colombia did not lead to a "race to the bottom" in local public expenditures.

The third essay studies whether governments incorporate economic efficiency considerations when choosing which firms they select for privatization. Analyzing mass privatizations following the Fall of the Berlin Wall in Germany, I employ previously unavailable firm data on more than 6,000 privatization and liquidation decisions. Within a descriptive analysis, I consistently find that privatization decisions indeed occurred in line with economic efficiency considerations, although not to a large degree. I do not detect evidence for

influence exerted through political patronage, close elections, or lobby group size. The analysis suggests that privatization decisions are less politicized and more efficiency-oriented than found in previous studies, highlighting the importance of institutional environments.

Keywords:
Public policy provision; decentralization; public employment services; fiscal spillovers; privatization.

Zusammenfassung

Über die letzten zwölf Jahre sind globale Indikatoren für Demokratie- und Freiheitsrechte kontinuierlich gesunken. Das Vertrauen in die Demokratie als Regierungsform ist an einem Tiefpunkt angelangt. Demokratie ist eine wichtige Triebkraft für wirtschaftliche Entwicklung, daher folgen aus diesem Vertrauensverlust auch Sorgen über die Zukunft des ökonomischen Wohlstandes. Als Ursache für diesen Vertrauensverlust haben Politikwissenschaftler unter anderem unwirksame politische Entscheidungsprozesse sowie eine schwache Politikgestaltung identifiziert. Diese Dissertation besteht aus drei Essays und untersucht zwei mögliche Reformen für effektivere politische Gestaltungsmöglichkeiten: Dezentralisierung und Privatisierung.

Das erste Essay überprüft, ob durch Dezentralisierung der öffentlichen Arbeitsvermittlung mehr Arbeitslose in freie Stellen vermittelt werden können. Dezentralisierung ist ein weit verbreitetes Instrument, welches Regierungen dabei unterstützen kann, die Wirksamkeit des staatlichen Handelns zu erhöhen. Allerdings sind mögliche Auswirkungen aus Sicht der ökonomischen Theorie uneindeutig und empirische Evidenz ist nur vereinzelt vorhanden. Durch die Kommunalisierung deutscher Jobcenter im Jahr 2012 entstanden innerstaatliche Unterschiede im Ausmaß der Dezentralisierung der Arbeitslosenvermittlung. Diese Unterschiede analysiere ich mit Hilfe eines Differenzen-von-Differenzen-Ansatzes. Dabei stelle ich fest, dass durch Dezentralisierung die Neuanstellung von Arbeitslosen um rund 10% reduziert wird. Es zeigt sich, dass dezentralisierte Arbeitsvermittlungen vermehrt öffentlich geförderte Arbeitsbeschaffungsmaßnahmen nutzen. Hierdurch wird die Aussicht auf Wiederbeschäftigung von Arbeitssuchenden gesenkt und zusätzliche Kosten für die zentralstaatlichen Kassen verursacht.

Das zweite Essay beschäftigt sich mit finanzpolitischen Wechselwirkungenn zwischen Gemeinden in Kolumbien. Hier wurde die Verantwortlichkeit für einen großen Teil des Staatsbudgets an die kommunale Regierungsebene übertragen. Ich überprüfe dabei, ob kommunale Ausgaben in benachbarten Gemeinden das Ausgabeverhalten kolumbianischer Bürgermeister beeinflussen. Dabei nutze ich eine quasi-experimentelle Identifikationsstrategie, welche Unterschiede zwischen den Gemeinden in Bezug auf das Vorhandensein von Bodenschätzen mit der Variation aus zeitlichen Schwankungen des Ölpreises an den Weltmärkten kombiniert. Es ergeben sich starke räumliche Autokorrelationen im lokalen Ausgabeverhalten. Allerdings zeigt sich im quasi-experimentelle Instrumentalvariablenansatz, dass es keine kausalen fiskalischen Interaktionseffekte zwischen den Gemeinden gibt. Für die Identifikation von echten fiskalischen Wechselwirkungen ist daher die Nutzung zusätzlicher Quellen exogener Variationen von höchster Bedeutung. Im Kontext der Entwicklungsforschung zeigen diese Ergebnisse, dass Dezentralisierung in Kolumbien nicht zu einem Unterbietungswettlauf in den kommunalen (Sozial-)Ausgaben geführt hat.

Das dritte Essay analysiert, ob Regierungen ökonomischen Effizienzüberlegungen folgen, wenn sie entscheiden, welche staatlichen Firmen für eine Privatisierung ausgewählt werden. Basierend auf der Massenprivatisierung in Folge des Falls der Berliner Mauer, untersuche ich Firmendaten, welche mehr als 6.000 Privatisierungs- und Liquidationsentscheidungen umfassen. Durch deskriptive Analysen zeige ich, dass Privatisierungsentscheidungen mit ökonomischen Effizienzüberlegungen zu vereinbaren sind. Ich finde hingegen keine Evidenz für politische Beeinflussung durch Klientelismus, knappe Wahlausgänge oder der Größe von potentiellen Lobbygruppen. Die Ergebnisse legen nahe, dass Privatisierungsentscheidungen weniger politisch, sondern stärker ökonomisch orientiert sind, als dies durch bisherige Studien bekannt ist.

Schlagwörter:
Politikgestaltung; Dezentralisierung; öffentliche Arbeitsvermittlung; fiskalische Ausstrahlungseffekte; Privatisierung.

To Irene

Acknowledgement

My dissertation greatly benefited from the generous support of numerous individuals and institutions. First and foremost, I would like to thank my two supervisors. Alexandra Spitz-Oener provided me with the opportunity to work at her institute and the freedom to explore a very diverse set of research questions. Her continuous support provided the foundation for all further developments during my PhD. I thank Rajshri Jayaraman and her red pencil for the most critical and helpful assessments I have received during my student life. Her intellectual challenge made me grow as a researcher.

Moreover, I am grateful to my co-authors Michael Weber, Frank Fossen, and Nicolas Pardo. Our joint work has spurred mutual learning, insightful discussions, and friendships. Among the current and former members of the Faculty of Business and Economics, I would like to highlight the helpful discussions with Michael Burda, Bernd Fitzenberger, Albrecht Glitz, and Felix Weinhardt. My colleagues at Humboldt University offered professional assistance and mental relief whenever necessary. Dawn Nichols of Humboldt University's language center contributed English language tutoring. Kristin Schwier and her colleagues provided excellent administrative support. Eva Mörk and Uppsala University hosted me during a research visit in April 2015.

Over the years, I had the opportunity to collaborate with several students which provided excellent research assistance. These include Djordje Dotlic, Stephanie Finkenwirth, Alexander Graf, Philipp Hundertmark, Jakub Kondek, Frederik Lettow, Alexander Rebmann, Oliver Wach. All of them reliably produced high-quality data work, literature research, and manuscript readings.

I gratefully acknowledge financial assistance through scholarships by the Wissenschaftszentrum Berlin and the Konrad-Adenauer Foundation, as well as travel funding by the Society for Economics and Management at Humboldt University Berlin. The Berlin Doctoral Program in Economics and Management Science and the Berlin Network of Labor Market Research offered a stimulating environment to leverage my research.

I presented my work at the following seminars and conferences, gaining valuable feedback: European Association of Labor Economists (EALE) 29th Annual Conference (scheduled), St. Gallen, Switzerland; Annual Meeting of the European Economic Association 2016, Geneva, Switzerland; invited seminar at IAB Nuremberg 2015, Nuremberg, Germany; Brown Bag Seminar 2015, Humboldt University

of Berlin, Berlin, Germany; Warwick Economics PhD Conference 2015, Coventry, United Kingdom; 8th IAB Graduate Workshop 2015, Nuremberg, Germany; 3rd Potsdam PhD Workshop in Empirical Economics 2014, Potsdam, Germany.

Lastly, I owe my greatest debt to my family and friends for their incredible support over more than five years. This dissertation would not have been possible without your encouraging words, practical support, and appreciation.

Munich, September 2018

Contents

List of Figures

List of Tables

1 Introduction

Democracy is on the retreat. After decades of expanding political freedom, numerous countries across the globe have entered a period of reversal. According to the Freedom House indicators, global political rights and civil liberties in 2017 sank to the lowest levels of the past twelve years (Freedom House, 2018). Even within established democracies, disillusion about their form of government looms large. Surveying more than 125,000 citizens in 50 countries, the Democracy Perception Index finds that 64% of the respondents living in democracies do not believe that their government is acting in their interest (Dalia Research, 2018). Stanford University's Larry Diamond (2015) coined the term "democratic recession" to characterize these trends.

Following Diamond's dissection of the Freedom House indicators, the prime reason for the crisis of democracy is bad governance, that is the process of policy making. Similarly, Fukuyama (2015) concludes that the legitimacy of democracies "depends less on the deepening of their democratic institutions than on their ability to provide high-quality governance". This conclusion is also consistent with the observation that, on average, authoritarian governments enjoy higher approval rates than democratic states, according to the Democracy Perception Index (Dalia Research, 2018). Hence, governments should be in search of reform options to restore the confidence of their citizens.

From an economics perspective, a democratic recession is alarming as democracy has been found to drive economic development (Persson and Tabellini, 2008). Even though a direct effect of democracy on economic growth is controversial, it is well-established that freely elected governments and civil liberties allow for higher levels of economic freedom and less political instability (for example refer to the meta-study by Doucouliagos and Ulubaşoğlu, 2008). In return, these conditions spur private investment and increase the capacity of governments (Acemoglu et al., 2014). Thus, a crisis of democracy may ultimately turn into a crisis of economies.

In this dissertation, I assess two policy responses addressing growing discontent with democracy. These responses promise to improve the institutional environment of public policy making and thereby increase the citizens' approval of democratic governments. The first policy response is decentralization, which denotes the delegation of responsibilities to lower levels of government. I study decentralization reforms in chapters 2 and 3 of this thesis. The second type of policy response is privatization. Privatization implies transferring government activities to the private

sector and is at the core of my final chapter.[1]

Decentralization is an attractive idea to bring public policy making closer to the population. It enables tailoring public policies to local needs more directly, reaps local information advantages, spurs competition among local jurisdictions, and facilitates voting politicians out of office for bad policies. Such reforms have therefore been a widespread policy prescription by the World Bank and other international policy consultants. Bardhan (2002) even goes as far as to note that "around the world in matters of governance, decentralization is the rage". However, decentralization may also entail serious drawbacks. These include the capture of local governments by special interest groups, a lack of professionalism within local administrations, or "races to the bottom" in the case of social policies.

The economics literature studying decentralizations has been pioneered by Musgrave (1959) who coined the term "fiscal federalism". This literature has been centered around Oates' (1972) model of the decentralization theorem. The theorem essentially states that a decentralized public good provision is superior if horizontal spillovers do not exist and local preferences for the public good vary across regions. While initial models in this strand of the literature assumed a benevolent government, fiscal federalism subsequently evolved to model more realistic political behaviors, including self-interested governments (Besley and Coate, 2003). Further models have been built which stress the importance of spillovers or the possibility that central governments adjust public good provisions to local preferences themselves. The variety of available models and assumptions from economic theory signify the difficulty of establishing an efficient task assignment across different levels of government. Consequently, careful empirical assessments of actual policy outcomes are required to complement the theoretical arguments and to get a better understanding of how decentralization may improve policy-making.

Embedded in this literature, chapter 2 studies the decentralization of public employment services (PES) in Germany. In 2012, 41 German local districts became the sole providers of job placement services and social assistance. This setting is particularly well-suited for an analysis as the majority of districts maintained a joint provision with the Federal Employment Agency (FEA), a government agency supervised by the Federal Ministry for Labour and Social Affairs. Public employment services are an important policy domain as the federal government spends more than 10% of its budget on these services.[2] While the decentralization literature commonly relies on cross-country comparisons, this setting allows studying decentralization within a common institutional environment and clearly defined treatment and control groups. Based on monthly district-level data provided by the

[1] Further policy responses are available but beyond the scope of this dissertation, such as implementing more direct-democratic procedures.

[2] Own calculations. Includes all spending items of the budget section 11, chapter 12, group 1 ("Leistungen der Grundsicherung für Arbeitssuchende"). Budget year 2011. Source: bundeshaushalt.de

FEA, I employ a difference-in-differences model to estimate the effect of decentralization on new job placements of unemployed persons. Consequently, I compare decentralizing districts as the treatment group with centralized districts as a control group in the period from five years before to five years after the introduction of the reform. I find that transferring the provision of public employment services to district governments decreases job placements of unemployed by about 10%. This finding is stable across a diverse set of alternative specifications and robustness checks. In addition to providing clean evidence on the impacts of decentralization, the study contributes an explorative analysis of potential channels which may explain the unexpected decrease of job placements. The analysis rules out that diffing monitoring strategies or disparate caseworker quantities and qualities explain the finding. Unemployed persons in decentralized regions also do not stop being placed in jobs outside their home district. However, I detect differential uses of active labor market policies, with decentralized providers assigning public job creation schemes to the unemployed more frequently. This program type is known to generate few job placements but may be attractive for local governments as program participants may support the production of local public goods and services. I conclude that decentralization of public employment services within the 2012 reform of German job centers did not improve public policy provision.

In chapter 3, I examine fiscal spillovers in spending behavior among neighboring local governments. Such spillovers may arise as a consequence of fiscal decentralization. Fiscal decentralization implies shifting fiscal resources and corresponding expenditure policies to local governments. More specifically, I study expenditure policy interactions among municipal governments in Colombia, a state which transferred the expenditure authority over a substantial budget share to the municipal level. Thereby, Colombia followed a trend which is common among numerous other developing countries. A major concern regarding this type of decentralization is that local governments may engage in a "race to the bottom" in social spending to avoid attracting people in need (Figlio et al., 1999). Measuring expenditure interactions has been challenging as actual policy interactions need to be separated from other spatially correlated factors driving spending decisions (Gibbons and Overman, 2012). In this paper, I propose a new strategy to determine the extent of spatial expenditure interactions by exploiting the Colombian allocation rules for fiscal resources. In particular, revenues from the exploitation of natural resources are a major budget component. The allocation rules assign them according to municipalities' endowments with commodities and current world market prices of the respective good. With oil being an important natural resource for Colombia, I combine variation in oil endowments across municipalities and inter-temporal variation in global oil market prices to build a novel instrument for municipalities' expenditure levels. Employing highly disaggregated spending data

for Colombian municipalities between 2000 and 2010 does not provide evidence for the "race to the bottom"-hypothesis or other forms of expenditure interactions. However, I demonstrate that traditionally used spatial econometric techniques, not relying on plausibly quasi-experimental variation, would yield strong evidence in favor of expenditure interactions among neighboring municipalities. My findings thus alleviate a common objection against decentralization as a policy option and underscore the need for credible identification strategies for measuring fiscal spillovers.

Chapter 4 deals with privatization as an alternative policy response to improve public policies and, if successful, to restore confidence in governments. Transferring government activities to the private sector may be beneficial as private enterprises have a larger incentive to increase the efficiency of policy provision. Profit motives and competition provide an environment which favors improvement and innovation. Privatization also formally makes policy provision less susceptible to potentially counterproductive political influences. However, the process of privatization is political itself and may determine privatization outcomes endogenously. Privatization literature is vast regarding studies of post-privatization effects while very few papers address the decision process leading up to privatization (Estrin et al., 2009). In chapter 4, I investigate the mass privatizations in East Germany following the Fall of the Berlin Wall in 1989 and analyze which firms were selected for privatization by the responsible government agency. After reunification there was a broad consensus within the government that the entirely state-owned East German manufacturing sector ought to be privatized. The highly uncompetitive conditions of East German firms and the corresponding need for restructuring made it infeasible to distribute sales subsidies on a scale such that all companies could be privatized successfully (Akerlof et al., 1991). Hence, the *Treuhandanstalt*, being the responsible government agency, had to decide which firms to privatize and which ones to liquidate. Using firm-level data on more than 4,000 firms I assess whether economic efficiency based on firms' initial productivity mattered for the decision of which firms to privatize. I find a modest but highly robust link between productivity and privatization. I do not detect evidence for political patronage, close elections or lobby sizes to play a role in these decisions.

In sum, this dissertation underscores the challenges in improving the provision of public policies. Decentralization and privatization raise high expectations. Nonetheless, to be effective, they depend upon careful implementation and adaptation to the specific institutional context. Unintended side effects may still arise making continuous monitoring and evaluation efforts indispensable. Otherwise, the cure might be worse than the disease.

The following three chapters are self-contained and may be read independently. Chapter 2 has been invited for resubmission to the *Journal of Public Economics*. It

is joint work with Michael Weber, though based on an earlier single-authored *BDPEMS Working Paper*. Chapter 3 is co-authored with Frank Fossen and Nicolas Pardo and published in the peer-reviewed *International Tax and Public Finance* journal. Chapter 4 is single-authored.

2 Public Employment Services Under Decentralization: Evidence from a Natural Experiment

2.1 Introduction

Governments commonly maintain public employment services that match job seekers with employers to increase reemployment rates. To improve the efficacy of these services, several countries, including Canada, Denmark, Germany, Italy, and Sweden, initiated reforms decentralizing responsibilities for public employment services to sub-national levels of government. These initiatives follow the classic theoretical argument that decentralized public employment offices are better informed about local economic conditions and preferences compared to a central agency. Hence, providing local governments with the autonomy to tailor labor market policies to these needs should result in superior policy outcomes (Oates, 1972; Faguet, 2004).

However, economic theory suggests at least three arguments why local policymakers could utilize their additional power for other objectives than reducing unemployment. First, they may aim to maximize their constituency's tax base by strictly focusing on job placements within their own region. This strategy would come at the cost of lower labor market mobility across regions and lead to fiscal externalities by creating a geographical lock-in of job seekers (Wildasin, 1991; Lundin and Skedinger, 2006). Second, local policymakers could strive to shift fiscal costs to other levels of government (Weingast et al., 1981; Besley and Coate, 2003). Thus, they might favor certain active labor market policies (ALMP) or monitoring strategies even if these policies are less effective in facilitating reemployment as long as they result in fiscal gains for the local constituency, for example because costs are covered by the national budget. Third, local policymakers seeking reelection may pressure decentralized employment services to ease welfare recipients' job search obligations (Brollo et al., 2015). This could also reduce the job-finding rate if public employment services at the local level are more susceptible to political influences than at the national level.

As economic theory is ambiguous, it is an empirical question whether centralized or decentralized regimes produce better employment services. This question has

remained unanswered due to empirical constraints, most importantly a lack of suitable control groups as the degree of decentralization usually varies between countries but not within them. If control groups were available, short program durations or simultaneous reforms obstructed the identification of causal effects (see Lundin and Skedinger, 2006; Boockmann et al., 2015).

In this paper, we address these challenges by exploiting a large-scale German policy experiment. This policy induced permanent within-country variation in the centralization of public employment services unimpaired by simultaneous reforms. The setting enables us to make two major contributions. For one, we provide clean evidence on the effect of decentralization on job finding. We thereby uncover important transition dynamics while tracking the decentralization effect over a period of four years. For another, we examine channels for this finding by analyzing changes in the main underlying activities of employment offices. These are providing job seekers and firms with placement services, managing active labor market programs (ALMPs), and monitoring job search efforts. In so doing, we provide an exploratory analysis to determine whether our findings are compatible with local governments following other idiosyncratic incentives that are not beneficial to job seekers.

Implemented in 2012, the German policy reform involved the devolution of public employment offices – referred to as 'job centers' hereafter – to the district level within 41 of Germany's 402 districts.[1] Job centers typically serve the long-term unemployed or people with very low earnings. For these groups, job centers play a crucial role in matching job seekers with potential vacancies (Pissarides, 1979; Graversen and van Ours, 2008; Fougère et al., 2009). Before the 2012 reform, individual job center policies were determined under the guidelines, directives, and supervision of the Federal Employment Agency (FEA), in cooperation with local authorities. After 2012, authorities of the 41 treated districts were free to independently manage and stipulate these policies. The financing of job centers remained unaffected by the reform. For all job centers, the federal government covered welfare benefits and costs for active labor market programs while local authorities funded accommodation costs.

We use this German policy reform to identify the causal effect of decentralizing job centers in a difference-in-differences framework. We implement the approach by estimating an aggregate stock-flow matching function using job centers that remained centralized as a control group (see Coles and Smith, 1998; Ebrahimy and Shimer, 2010). Our analysis employs an aggregate administrative dataset comprising the monthly stocks and gross flows of unemployed welfare recipients and vacancies in German districts from 2007 to 2016. The data further provides detailed information on ALMP inflows, enforcement of job-search requirements,

[1] German districts (*Kreise und kreisfreie Städte*) are an administrative subdivision similar to counties in the US. Job centers are organized at the district level.

and job quality indicators. We find that decentralization decreases the number of new job matches by roughly 17% in the first year and up to 10% during the second to fourth post-reform years. This effect is equivalent to an increased average unemployment duration of three months. We run a battery of robustness checks including placebo tests and triple-difference models that all support our results being driven by decentralization rather than confounding factors.

Having established this robust negative effect on job finding, we explore whether decentralization caused a geographical lock-in of job seekers or other changes in the job centers' placement, ALMP, and monitoring strategies that could account for these losses. Most importantly, we identify an immediate and permanent shift towards public job creation programs that are ineffective in increasing reemployment rates compared to other measures (see, for instance, Card et al., 2017). Decentralized job centers also temporarily reduced the enforcement of job seeker obligations, possibly reflecting transition processes rather than actual strategic changes. We do not find evidence for geographical lock-in effects. Moreover, we find no evidence that decentralization altered their placement strategies which would have altered placement quality in terms of job stability. Due to legal restrictions, we also rule out job centers having increased their employees' caseloads or employed caseworkers with different backgrounds and skills.

We conclude that job seekers did not benefit from decentralization. Decentralized job centers adjust labor market policies but in a way that does not improve job seekers' reemployment prospects as exemplified by the increase in ineffective job creation programs. Decentralized job centers potentially favored these schemes because they generate local public goods whereas the federal government covers most of the associated costs. Our findings have important consequences for public budgets. Via the lower job finding rate alone, our estimates imply that the 2012 decentralization caused additional fiscal costs of about 500 million euros. Hence, our study emphasizes that decentralization reforms necessitate a careful assessment of potential incentive problems and fiscal externalities to avoid unintended consequences.

This paper speaks to two strands of literature. First, it contributes to fiscal federalism research that has analyzed whether states should provide public goods and services at a centralized or decentralized level (see Geys and Konrad, 2010, for a review). Thus far, this literature has almost exclusively investigated decentralization with respect to public finances, education policies, environmental policies or political institutions.[2] Little attention has been paid to labor market institutions (Martinez-Vazquez et al., 2017). This gap is surprising given that policymakers

[2]See, for example, Baicker and Gordon (2006); Neyapti (2010); Baicker et al. (2012) for public finance, Barankay and Lockwood (2007); Ahlin and Mörk (2008); Galiani et al. (2008) for education policies, Sigman (2002); Banzhaf and Chupp (2012); Lipscomb and Mobarak (2017) for environmental policies, and Blanchard and Shleifer (2001); Enikolopov and Zhuravskaya (2007); Fan et al. (2009) for political institutions.

worldwide have pressed ahead promoting the decentralization of labor market institutions on a large scale. Second, we address the labor economics literature dealing with individual job matching instruments. This literature has made great progress in credibly identifying causal effects of active labor market policies (e.g. Black et al., 2003; Blundell et al., 2004; Card et al., 2010; Crépon et al., 2013) but remained agnostic about the institutional environment. In particular, it has remained silent on the question under which level of centralization such services should be delivered.[3]

Two studies have started to address these problems. Lundin and Skedinger (2006) study a Swedish pilot reform that granted municipal authorities a voting majority in the local employment committees, the bodies responsible for designing local labor market policies. The authors find that municipalities subsequently organized more ALMP projects and hard-to-place job seekers more likely enrolled in municipal projects. Remarkably, the official program period lasted only for three months, which was too short for employment outcomes and longer-lasting effects to be studied. Boockmann et al. (2015) examine a partial decentralization of German public employment offices from 2005 (see also Holzner and Munz, 2013) and find a negative effect of decentralization on the job-finding rate of men. Unfortunately, the empirical setting was constrained by a landmark unemployment benefit reform that directly coincided with the decentralization process. In contrast to these papers, our study has the following advantages. We observe the decentralization effect over a period of five years, are able to examine employment as well as local labor market policies, and our setting is not impaired by simultaneous reforms.

The chapter proceeds as follows. Section 2.2 provides details on the German system of public employment services and its 2012 reform. Section 2.3 describes the data and our empirical strategy. Section 2.4 presents the estimated effects of decentralization, and section 2.5 explores underlying channels. Section 2.6 examines the validity of these results and Section 2.7 concludes.

2.2 Policy Background

2.2.1 German Job Centers

German job centers are one-stop local employment offices that play a central role in the German welfare system. As of January 2012, they have served 2 million long-term unemployed job seekers and 2.4 million employed workers with very low

[3] A small number of papers have compared public to private provision regimes , finding mixed results for job seeker-outcomes (see, for instance Heinze et al., 2006; Bennmarker et al., 2013; Behaghel et al., 2014).

labor incomes, or 8% of the Germany's working age population.[4] Their clients' poor labor market prospects give job centers a major role in welfare-to-work transitions (see, for instance, Fougère et al., 2009). Job centers engage in job counseling and assign clients to jobs or ALMP measures. They also monitor their clients' job search efforts and may temporarily impose cuts on unemployment benefits if a job seeker does not comply with their job-seeker obligations. These include actively searching for a new job, meeting with their caseworkers, participating in assigned ALMP measures, and accepting appropriate job offers. According to the social security code, the aim is to allow clients a life in dignity and integrate them into employment.

Unique to Germany, two types of job centers exist that vary in their degree of local autonomy as portrayed in Table 2.1. The first column introduces centralized job centers (*gemeinsame Einrichtungen*), which are governed by the Federal Employment Agency (FEA) in cooperation with the respective district authority. In charge of all labor market integration tasks, the FEA supervises the local employment offices using target agreements, directives, and technical supervision such that the provision of public employment services is comparatively standardized across centralized job centers. In particular, placement, ALMP, and sanction policies follow nationwide guidelines with limited strategic leeway for local adjustments. The district administration mainly provides social inclusion services, for instance in the case of drug addiction or psychological problems.

	Centralized	Decentralized
Task responsibilities		
Placement services	FEA	District
Social inclusion services	District	District
ALMP assignments	FEA	District
Monitoring & sanctions	FEA	District
Governance		
Affiliation	FEA & district	District
Target agreements	With FEA	With state authorities
Technical supervision	FEA	Customized
Financing		
Unemployment benefits	Federal government	Federal government
ALMP measures	Federal government	Federal government
Accommodation costs	District	District

Notes.– FEA: Federal employment agency. ALMP: Active labor market programs.
Sources.– Ruschmeier and Oschmiansky (2010); Boockmann et al. (2015).

Table 2.1: Job Centers by Type of Organization

[4]Job centers serve the residents of their district. Six job centers serve multiple districts, covering 16 districts in total.

The second type of job center is decentralized with district administrations assuming responsibility for all employment services (*zugelassene kommunale Träger*, second column of Table 2.1). Unlike their centralized counterparts, these job centers operate completely independently of the FEA except for the exchange of unemployment registration data. Decentralized job centers constitute a regular part of the district administration led by the district mayor. There is no general technical supervision by the FEA. District governments only sign target agreements with their respective state governments, their sole de-jure supervisors.

Both job center types share a common legal framework and financing rules. The federal government covers unemployment benefits and expenditures for labor market programs of job-center clients while the local administrations finance their accommodation. The autonomy of decentralized job centers with regard to placement, ALMP, and sanction strategies potentially allows for a better adjustment to local labor market conditions which could improve job finding. However, the financing structure could incentivize local decision makers to implement strategies that are primarily beneficial for local budgets rather than job seekers.

Beyond job centers, the government runs an unemployment insurance scheme managed by the federal employment agency and financed by contributions of employers and employees. Benefits are available to previous contributors for a period of 12 months at most. For further support, insurance recipients will be redirected to job centers. Thus, job centers are responsible for about two thirds of all unemployed persons in Germany.

2.2.2 The 2012 Decentralization

Decentralized job centers were established in two waves. The first wave, in 2005, established job centers as one-stop employment offices for the first time into the welfare system whereby decentralized job centers were set up in 67 districts of Germany's 402 districts. This wave also coincided with a large-scale reform of long-term unemployment benefits (Hartz reform, see Dustmann et al., 2014; Nagl and Weber, 2016).[5] The second wave, in 2012, devolved job centers in 41 other districts. This decentralization provides a pre-reform period and took place without other simultaneous labor market reforms. For these reasons, we focus our analysis entirely on the second wave of reform.

The districts to be reformed in 2012 were determined within a state-quota system. Districts willing to decentralize first had to apply to their respective state governments. The application period started on 3 August 2010 and ended on 31 December 2010. Local councils were required to back the application with a

[5] An official evaluation of this decentralization wave led to inconclusive results (Deutscher Bundestag, 2008; Holzner and Munz, 2013; Boockmann et al., 2015), such that no political consensus was reached about the preferred regime. As a compromise, the co-existence of centralized and decentralized job centers was continued.

two thirds majority vote. Then, the state governments nominated those applicants allowed to decentralize. The number of nominations was subject to a quota specific to each state, proportional to the state's number of delegates in the upper house of parliament. The total quota for Germany as a whole was 41 districts. 75 districts applied. If the number of applying districts fell short of the available spots in one state, remaining places were filled by districts from other states. Those districts allowed to decentralize their job centers were officially announced on 14 April 2011. Decentralization took place on 1 January 2012.

Thanks to the state-quota system, job centers were decentralized in districts all across Germany (see Figure 2.1). They do not cluster in regions with particularly poor or strong labor market conditions, nor are they disproportionally located in cities or rural areas. They also resemble one another in more general economic indicators. Table 2.2 presents major district characteristics by job center type for the pre-application year 2010. As shown in the comparison of means, both groups exhibited on average the same gross domestic product, fiscal situation, population size, sectoral structure, and unemployment composition. A difference arises only for the monthly job-finding rate. As we use job finding solely as an outcome variable in our framework, district fixed-effects will account for these differences in our estimations. In sum, these results supply first evidence that the two groups of districts are observationally equivalent.

2.3 Data and Empirical Strategy

2.3.1 Data

We utilize a rich administrative dataset at the district level to examine the effects of decentralization on job finding and other labor market outcomes. The data stem from the job centers' operational processes and are subsequently compiled into monthly reports (*Arbeitsmarkt in Zahlen*) by the FEA's statistical office. The reports provide monthly observations on unemployment, vacancies, ALMP participation, benefit sanctions, and employment quality indicators. For all variables, we readily observe stocks as well as gross flows and thus do not have to deal with time aggregation issues.

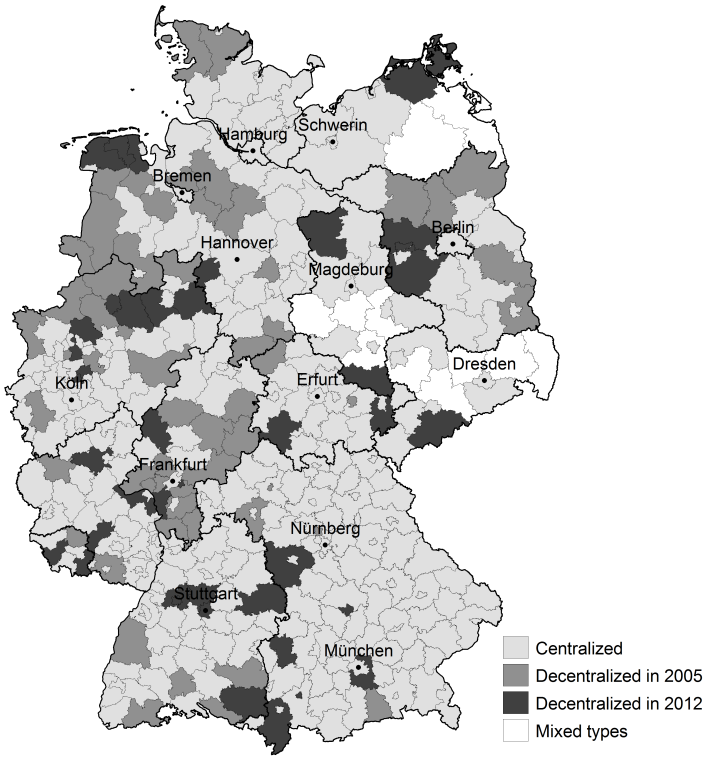
The data cover the universe of German districts and effectively consider all relevant unemployed job seekers because unemployment registration is mandatory for receiving unemployment benefits. From this sample, we omit 11 districts in which centralized and decentralized job centers co-existed due to administrative reforms. This also includes one district which was part of the 2012-reform, leaving us with a total of 40 treated districts. Finally, we omit districts that decentralized job centers in 2005 although their inclusion as an additional control group does

Variable	Group means: Decentralized in 2012	Group means: Remained centralized	P-Value for equality of means
GDP per capita (in 1,000 euros)	29.670	29.085	0.766
	(14.831)	(11.196)	
Public debt p.c. (in 1,000 euros)	1.725	1.587	0.518
	(1.403)	(1.248)	
Urban district (dummy)	0.225	0.313	0.257
	(0.423)	(0.464)	
East Germany (dummy)	0.200	0.173	0.681
	(0.405)	(0.379)	
Civil labor force (in 1,000)	154.230	131.493	0.432
	(96.583)	(179.278)	
Employment rate	0.724	0.763	0.269
	(0.170)	(0.216)	
Share: Agriculture	0.021	0.023	0.527
	(0.019)	(0.021)	
Share: Mining and energy	0.014	0.013	0.904
	(0.008)	(0.009)	
Share: Manufacturing	0.204	0.195	0.535
	(0.086)	(0.087)	
Share: Construction	0.066	0.066	0.926
	(0.024)	(0.025)	
Share: Trade, transp., comm.	0.254	0.251	0.632
	(0.042)	(0.039)	
Share: Finance and real estate	0.142	0.141	0.824
	(0.045)	(0.046)	
Share: Public and priv. services	0.299	0.310	0.285
	(0.056)	(0.065)	
Job-center unemployment rate	0.048	0.047	0.812
	(0.025)	(0.029)	
Share: Young (15–24 years)	0.080	0.078	0.426
	(0.016)	(0.020)	
Share: Old (55–64 years)	0.123	0.124	0.843
	(0.028)	(0.022)	
Share: Foreign nationals	0.190	0.165	0.139
	(0.130)	(0.093)	
Monthly job-finding rate	0.042	0.047	0.041**
	(0.011)	(0.014)	
Monthly flow rate into ALMP	0.148	0.162	0.105
	(0.043)	(0.051)	
Monthly sanctioning rate	0.018	0.019	0.197
	(0.006)	(0.006)	
Observations	40	294	

Notes.– Sample as described in section 2.3.1. Standard deviations in parentheses. P-values given for t-test of mean equality. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.
Source.– German Statistical Office and Federal Employment Agency.

Table 2.2: Major District Characteristics in 2010 by Job Center Type

Figure 2.1: German Districts by Job Center Type



Notes.– Mixed types refer to districts where decentralized and centralized job centers coexist e.g. due to district mergers.
Sources.– Geodata: GeoBasis-DE / BKG 2014.

not alter our results. The sample period ranges from January 2007 to December 2016, i.e. from five years before to five years after the decentralization. Due to partially missing observations, we remove the first three post-reform months from our sample.

The data allow us to study the effect of decentralization on a wide range of labor market outcomes. With job placements being the primary task of job centers, we consider the monthly outflow from unemployment into employment as our main outcome of interest. To investigate channels explaining potential changes in unemployment outflows, we analyze several additional outcomes. These include the monthly flows of unemployed into different types of ALMP measures, the number of sanctions issued in a month on non-compliant job seekers, and the outflow into permanent jobs. The latter refers to jobs lasting for at least three months. Our main covariates are stocks and flows of unemployed and vacancies, respectively. In additional specifications, we also consider the demographic composition of local unemployment, such as the share of foreign nationals, people younger than 25 years, and older than 55. Table 2.3 presents descriptive statistics of our main variables in the resulting sample; Table A.1 in the appendix provides respective variable descriptions.

Variable	Mean	SD	Min.	Max.
Monthly job-finding	4.67	0.89	1.61	8.95
Vacancies, inflow	5.75	0.76	3.09	9.65
Vacancies, stock	6.71	0.81	2.94	10.47
Unemployed, inflow	6.30	0.89	3.64	10.68
Unemployed, stock	7.96	1.02	5.48	12.29
Share: Unemployed <25 yr	7.74	2.13	0.06	30.58
Share: Unemployed >55 yr	14.59	4.08	3.44	31.21
Share: Foreign nationals	18.32	10.49	0.34	59.43
Flow into ALMP	5.90	1.03	2.20	10.22
Into short-term training	5.40	1.04	0.41	9.47
Into subsidized employment	2.85	1.18	0.41	7.51
Into medium-term training	3.16	1.51	0.41	8.78
Into public job creation s	3.66	1.47	0.41	9.22
New sanctions	4.20	0.99	0.41	8.97
Stock of sanctions	4.81	0.98	0.41	9.48
Stock of benefit sanctions	4.76	0.99	0.41	9.45
Stock of accomodation sanc	2.64	1.03	0.41	6.95
Outflow out of welfare	5.68	0.86	2.08	9.86
Permanent outflow out of w	5.33	0.87	1.61	9.59
Share: Permanent outflow	70.63	6.31	31.90	94.38

Notes.– Monthly district-level data. All level-variables are in logs. $N = 39{,}018$.

Table 2.3: Descriptive Statistics of Main Regression Variables

2.3.2 Econometric Model

The functional form of our econometric model is motivated by a stock-flow matching model with Cobb-Douglas technology (Coles and Smith, 1998; Ebrahimy and Shimer, 2010).[6] Analogous to a production function, the stock-flow matching function models the gross flow from unemployment into jobs ('matches') as an output produced by the stocks of vacancies and unemployed as well as their respective inflows. We interpret the total factor productivity of the matching function as an indicator for the efficiency of the local job center in bringing unemployed back to work. The decentralization status of a job center then constitutes one component of this indicator.

To identify the causal effects of decentralization, we employ a difference-in-differences framework at the district level. Our treatment group comprises 40 districts whose job centers were decentralized in 2012, while our control group contains 294 districts whose job centers remained centralized throughout the sample period.

[6]The stock-flow matching function has received empirical support both at the micro and the macro level (Gregg and Petrongolo, 2005; Andrews et al., 2013) with strong evidence for a Cobb-Douglas functional form (see Petrongolo and Pissarides, 2001, for a survey).

Log-linearizing the stock-flow matching function, our estimation equation then reads

$$M_{it} = \delta D_{it} + \beta_1 U_{it} + \beta_2 V_{it} + \beta_3 \widetilde{U}_{it} + \beta_4 \widetilde{V}_{it} + \alpha_i + \mu_t + \varepsilon_{it} \tag{2.1}$$

where M_{it} denotes matches defined as transitions from unemployment into jobs for district (i.e. job center) i and month t, our main outcome. The dummy variable D_{it} indicates whether a job center is decentralized or not. U_{it} and V_{it} denote the stocks of unemployed and vacancies, whereas $\widetilde{U}_{it}$ and $\widetilde{V}_{it}$ denote their respective inflows in this month. We include district-specific effects α_i to account for time-invariant differences in matches across districts and month-fixed effects μ_t to capture business cycle and seasonal fluctuations. Our parameter of interest is δ, which provides the treatment effect of decentralization on the conditional outflow from unemployment to employment. Standard errors are clustered by district and month to account for unobserved correlation within these dimensions (Bertrand et al., 2004).

Our empirical approach relies on two main identifying assumptions.[7] First, centralized and decentralized job centers experience the same fundamental labor market trends in the absence of the policy change. Second, decentralization has no effect on job finding in unreformed districts (stable unit treatment value assumption, SUTVA). We find descriptive support for common trends in the following section and more formal support for both assumptions in Section 2.6.

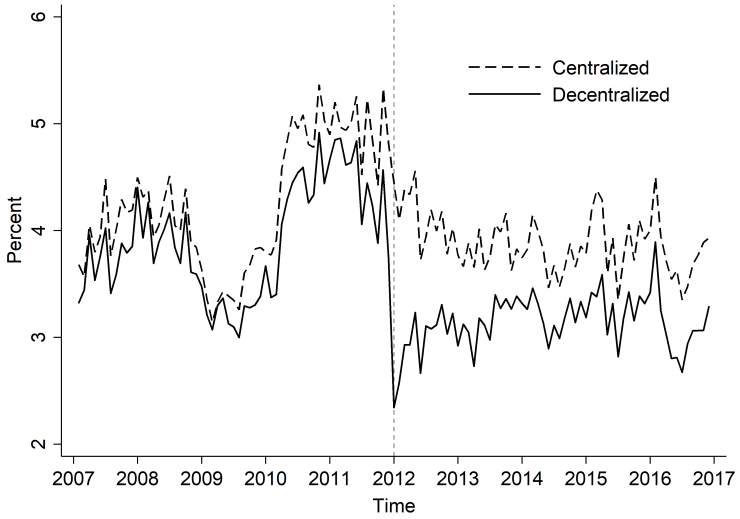
2.4 The Effects of Decentralization on Job Finding

This section presents descriptive, static parametric, and dynamic parametric results for the effect of decentralization on job finding. Illustrating the effect of decentralization descriptively, Figure 2.2 shows the evolution of the seasonally adjusted average aggregate monthly job-finding rates by job center type over time. The job-finding rate is calculated as the outflow out of unemployment over the unemployment stock at the beginning of the month. The figure illustrates that in the five years before the reform, the job-finding rates' evolution was remarkably similar in both groups of job centers. This lends credibility to the common trends assumption and affirms that the reform did not target districts with particularly bright pre-treatment trends. We plot descriptive graphs for further labor market outcomes in Figures A.1 and A.2 in Appendix A.2, again confirming parallel pre-reform trends. However, after the decentralization in January 2012, the job-finding rate of decentralized job centers sharply dropped relative to centralized job centers. It declined from about 4.5% to roughly 3% in treated districts and from approximately 5% to around 4%

[7] We have already discussed in Section 2.2 that the 2012 decentralization did not coincide with other reforms that could have affected the two groups of districts systematically differently.

in non-treated districts. The gap slightly narrows during the following years, but does not return to its pre-reform size. This points to permanent negative effects of decentralization on job finding.

Figure 2.2: Average Aggregate Monthly Job-finding Rates by Job Center Type



Notes.– The figure depicts the seasonally adjusted average aggregate monthly job-finding rate. It is calculated as the monthly outflow out of unemployment into employment over the unemployment stock at the beginning of the month. The time-labels (x-axis) refer to January of a given year.

We now investigate whether parametric estimates will support our descriptive findings. Table 2.4 reports the difference-in-differences estimates from equation (2.1) for the outflow from unemployment into employment. Each column represents a regression of log transitions into jobs on a decentralization indicator, district and month fixed effects, as well as subsequently introduced covariates. Column 1 gives the average treatment effect of decentralizing job centers while controlling only for fixed effects. The estimate implies that average monthly flows into jobs decreased by roughly 11% due to decentralization. Columns 2 to 4 refine the model's precision by adding a set of local labor market characteristics that remove cross-district differences. In particular, column 2 adds the monthly stocks of vacancies and unemployed. Building on column 2, column 3 includes the respective inflows, completing the basic stock-flow model. The coefficients of the stock-flow variables are in line with the concept of stock-flow matching. Job finding is more elastic with respect to the inflows of new vacancies rather than its stock, while it is more elastic with respect to the stock of unemployed rather than its inflow. The decentralization effect remains robust and stable. Column 4 additionally controls for shares of three demographic groups that are typically hard to place into jobs, i.e. the share of unemployed below the age of 25, the share of unemployed above the age of

55, and the share of foreign unemployed. As expected, higher shares of these hard-to-place job seekers in the group of unemployed *ceteris paribus* reduce the unemployment outflow into employment. Yet, controlling for these groups does not alter our decentralization estimate. Our finding is also robust to including linear district-specific trends into the empirical model and using alternative sample periods (see Tables A.2 and A.3 in Appendix A.3).

Hence, we conclude that decentralization reduced the monthly flow into jobs on average by about 10% within five years following the reform. This effect size is equivalent to an increase in the average unemployment duration by about three months.[8]

Variable	(1) Fixed Effects	(2) Stocks	(3) Stock-Flow	(4) Controls
Decentralized	−0.119 ***	−0.124 ***	−0.096 ***	−0.100 ***
	(0.028)	(0.025)	(0.021)	(0.021)
Vacancies, stock		0.029 **	−0.027 **	−0.025 **
		(0.013)	(0.011)	(0.011)
Unemployed, stock		0.562 ***	0.372 ***	0.383 ***
		(0.036)	(0.031)	(0.031)
Vacancies, inflow			0.110 ***	0.111 ***
			(0.012)	(0.012)
Unemployed, inflow			0.331 ***	0.327 ***
			(0.021)	(0.021)
Unemployed <25 ys				−0.004 **
				(0.002)
Unemployed >50 ys				−0.005 ***
				(0.002)
JC unemployed: foreign citizens				−0.004 **
				(0.002)
R-squared	0.951	0.955	0.958	0.958
Districts	334	334	334	334
Observations	39018	39018	39018	39018

Notes.– * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. Each column presents a different estimation of equation (2.1). *Decentralized* is a dummy equaling 1 for districts with decentralized job centers and 0 otherwise. All continuous variables in logs. Regressions include a full set of dummies for districts and months. Standard errors given in parentheses are clustered at the job center and the month level.

Table 2.4: Difference-in-Differences: Average Effect of Decentralization on Monthly Log Flows Into Jobs

[8]The average aggregate monthly job-finding rate in centralized districts amounts to 3.8%. Assuming a constant job-finding probability over the duration in unemployment, this implies an average unemployment duration of about 26 months. A 10% decrease of job finding, therefore, implies an increase in average unemployment duration by almost 3 months.

Next, we investigate whether the negative effect of decentralization is declining over time, as Figure 2.2 may suggest. We therefore modify the stock-flow matching model from equation (1). Adding a full series of annual leads and lags of the reform, the regression equation now reads

$$M_{it} = \sum_{\substack{\tau=2007 \\ (\tau \neq 2011)}}^{2016} \delta_\tau D_{i\tau} + \beta_1 U_{it} + \beta_2 V_{it} + \beta_3 \widetilde{U}_{it} + \beta_4 \widetilde{V}_{it} + \alpha_i + \mu_t + \varepsilon_{it} \tag{2.2}$$

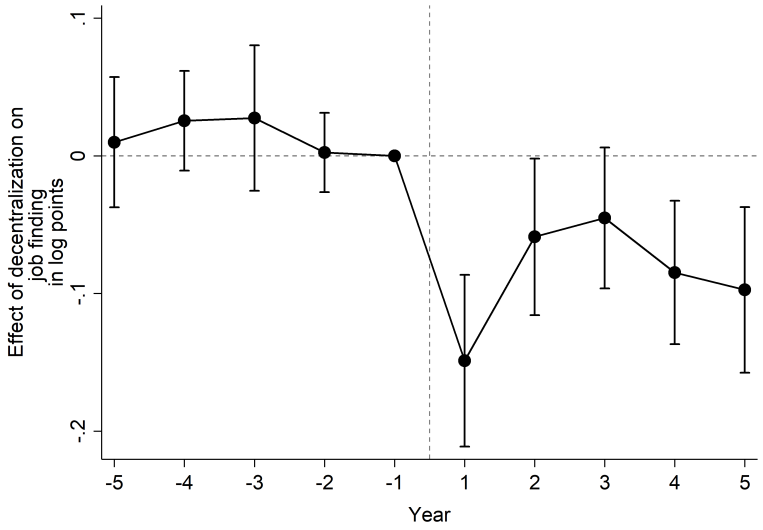
where τ denotes years and δ_τ are yearly coefficients. As anticipatory treatment effects could have occurred already when the decentralizing districts were announced in April 2011, all treatment effects are estimated relative to the pre-treatment base year 2011. Estimating quarterly effects leads to qualitatively similar but less precise results.

Figure 2.3 depicts the resulting evolution of the decentralization effect on job finding from five years before to five years after the reform. During the pre-reform period, all coefficients are statistically insignificant. This finding rules out anticipatory decentralization effects and supports the common trends assumption underlying our identification strategy. In the first year after decentralization, monthly unemployment outflows were strongly reduced by about 17%. During the following three years, this effect weakens over time but still amounts to almost 10% in the fifth year after decentralization. Seven expert interviews with division heads of state and federal ministries as well as job center directors suggest that the first year after the reform was influenced by the organizational transition. Employees had to adapt to new IT systems and practices. Moreover, some functions that used to be centrally provided by the FEA before had to be built up in decentralized job centers. We thus conclude that decentralization initiates a transition phase with a particularly pronounced drop in the job finding rate during the first year but also induces a more permanent and economically relevant negative effect in subsequent years that requires explanation.

2.5 Policy Adjustments

We now explore channels that might explain the negative effect of job center decentralization on job finding. As argued above, decentralization may lead to (i) a geographical lock-in of job seekers; (ii) changes in the inflows into ALMP measures; (iii) changes in the monitoring and sanctioning of job seekers or (iv) changes in the placement strategy. Finally, we will briefly discuss additional features of public employment services playing a potential role in the decentralization process.

Figure 2.3: Dynamic Treatment Effects of Decentralization on Monthly Flows Into Jobs



Notes.– The figure depicts coefficients and their 95%-confidence intervals of yearly leads and lags of the decentralization indicator from a stock-flow regression of the log monthly flow from unemployment into jobs, as given by equation (2.2). The year 2011 is the baseline category. The regression includes a full set of dummies for job centers and months. Standard errors are clustered at the job center and the month level.

2.5.1 Geographical Lock-in of Job Seekers

If local decision makers aim to maximize the tax base of their constituency, they have an incentive to match job seekers only with vacancies in their own district. This would lead to a lower mobility of job seekers across districts and could explain a lower job finding rate under decentralization (Lundin and Skedinger, 2006). This phenomenon has been termed as 'geographical lock-in' of job seekers and could create an uncoordinated fiscal externality among districts (Wildasin, 1991).

To examine whether decentralization induces geographical lock-in, we analyze whether the elasticity of job finding with respect to vacancies from surrounding districts has decreased after decentralization. Therefore, we extend our previous model (2.1) by adding spatial lags of all variables as well as interaction terms of the spatial lags with the decentralization dummy as covariates. This gives rise to a spatial cross-regressive model (Halleck Vega and Elhorst, 2015). Hence, we estimate models of the form

$$M_{it} = \delta D_{it} + Q_{it}\beta + WQ_{-it}\gamma + WD_{-it}\eta + D_{it}WQ_{-it}\theta + \alpha_i + \mu_t + \varepsilon_{it} \tag{2.3}$$

where D is the decentralization indicator, Q is a vector collecting the stock and flow variables for unemployed as well as vacancies, and W represents a spatial weights matrix based on row-normalized inverse distances. The remaining variables are

defined as before. Our coefficient vector of interest now becomes θ, in particular its elements with respect to vacancies. The index i denotes the focal district, whereas $-i$ refers to the 'neighbors' of district i. To provide a meaningful interpretation of the decentralization coefficient δ in the face of interaction terms, we center all continuous variables around their mean and standardize them by their standard deviation.

Variable	(1) Baseline	(2) Spatial lags I	(3) Spatial lags II	(4) Spatial lags full
Decentralized (D)	−.096 ***	−.107 ***	−.103 ***	−.097 ***
	(.021)	(.021)	(.024)	(.022)
W×Vacancies, inflow		.074 ***	.074 ***	.077 ***
		(.024)	(.024)	(.024)
W×Vacancies, stock		−.025	−.025	−.023
		(.031)	(.031)	(.030)
D×W×Vacancies, inflow		−.000		−.014
		(.013)		(.017)
D×W×Vacancies, stock			−.005	−.004
			(.015)	(.020)
R-squared	.958	.959	.959	.959
Districts	334	334	334	334
Observations	39018	39018	39018	39018

Notes.– * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. Each column presents a separate estimation of equation 2.3. *Decentralized* (*D*) is a dummy equaling 1 for districts with decentralized job centers and 0 otherwise. *W* represents a spatial weights matrix with row-normalized inverse distances as weights. Regressions include the stocks and flows of unemployed and vacancies as well as a full set of dummies for job centers and months. All continuous variables in logs, centered and standardized. Standard errors given in parentheses are clustered at the job center and the month level.

Table 2.5: Difference-in-Differences: Average Effect of Decentralization on Flows Into Jobs Accounting for Vacancies in Surrounding Job Centers

Table 2.5 presents our results. The first column repeats our baseline estimation using the centered and standardized variables, demonstrating that the decentralization effect remains unaffected by this transformation. Column 2 adds the spatially lagged variables as well as an interaction of the spatially lagged vacancy inflow with the decentralization indicator. Column 3 substitutes this interaction with that of the spatially lagged vacancy stock with decentralization. Column 4 includes a full set of interactions of spatially lagged stock and flow variables with the decentralization indicator. In all models, the resulting mean decentralization effect is very similar to our previous estimates. Job finding increases with additional vacancy inflows in surrounding districts but not with vacancy stocks. None of the models indicates a statistically significant decrease of the job finding elasticity with respect to the neighboring stock or inflow of vacancies after decentralization. Confirming

the results by Lundin and Skedinger (2006), we conclude that decentralization did not increase the geographical lock-in of job seekers. Instead, decentralization appears to have reduced the overall efficiency of the job matching process.

2.5.2 Active Labor Market Policies

Changes in the assignment of job seekers into ALMP measures constitute a second potential channel explaining the reductions in job finding. Decentralized job centers could use their autonomy to better tailor ALMP strategies to local economic conditions. Yet they may also promote program types that provide additional gains for the local constituency such as local public goods. Similarly, Lundin and Skedinger (2006) point out that decentralized job centers might prefer ALMP measures that help to maximize the local tax base, even if they came at the cost of higher geographical lock-in. However, an ALMP strategy that does not focus on the most effective programs for increasing reemployment rates will reduce the aggregate job-finding rate.

For German job centers, the four most common ALMP categories are short-term classroom and on-the-job training of up to 3 months (*Aktivierung und berufliche Eingliederung*), medium-term vocational training and re-training (*Berufliche Weiterbildung*), wage subsidies (*Aufnahme einer Erwerbstätigkeit*), and public job creation schemes (*Beschäftigung schaffende Maßnahmen*). For all these measures, the federal government bears the cost of program participation.[9] Yet only public job-creation schemes offer the additional advantage of participants providing local public goods, such as cleaning streets, gardening parks or supporting local facilities' management. A shift towards public job-creation schemes could therefore reduce the districts' own expenditures for these goods. Unfortunately, public job-creation schemes are also very ineffective in increasing reemployment rates compared to other measures (for large-scale meta-studies, see Heckman et al., 1999; Kluve, 2010; Card et al., 2017). Wapler et al. (2018) even show this program type to reduce the regional matching efficiency between job seekers and vacancies. Simple ordinary least squares estimates using our sample indeed confirm that job finding is not or only vaguely correlated with previous inflows into public job-creation schemes (see Figure A.3 in Appendix A.4). Conversely, previous inflows into short-term training and wage subsidy programs exhibit strong positive correlations with job placements.

To assess whether decentralization caused a shift toward less effective ALMP measures, we employ the stock-flow model from equation (2.1) but use outflows from unemployment into the different ALMP programs as the outcome variables. Table 2.6 presents the respective results. The first column indicates that decentralized job centers do not assign their clients more or less often to ALMP measures

[9] District authorities mainly pay for accommodation costs of job seekers, see Section 2.2.

Variable	(1) All ALMPs	(2) Short-term trainings	(3) Medium-term trainings	(4) Wage subsidies	(5) Job creation schemes
Decentralized	0.033	−0.068	−0.060	−0.047	0.299 ***
	(0.064)	(0.096)	(0.069)	(0.073)	(0.080)
R-squared	0.939	0.860	0.810	0.845	0.825
Districts	316	316	316	316	316
Observations	36972	36967	35318	36422	35575

Notes.– * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. Each column presents a different estimation of equation 2.1. The dependent variables are inflows of unemployed into the respective ALMP categories. *Decentralized* is a dummy equaling 1 for districts with decentralized job centers and 0 otherwise. Regressions include the stocks and flows of unemployed and vacancies as well as a full set of dummies for job centers and months. All continuous variables in logs. Sample sizes vary due to missing observations. Standard errors given in parenthesis are clustered at the job center and the month level.

Table 2.6: Difference-in-Differences: Average Effect of Decentralization on Monthly Log Flows Into Active Labor Market Policies (ALMPs)

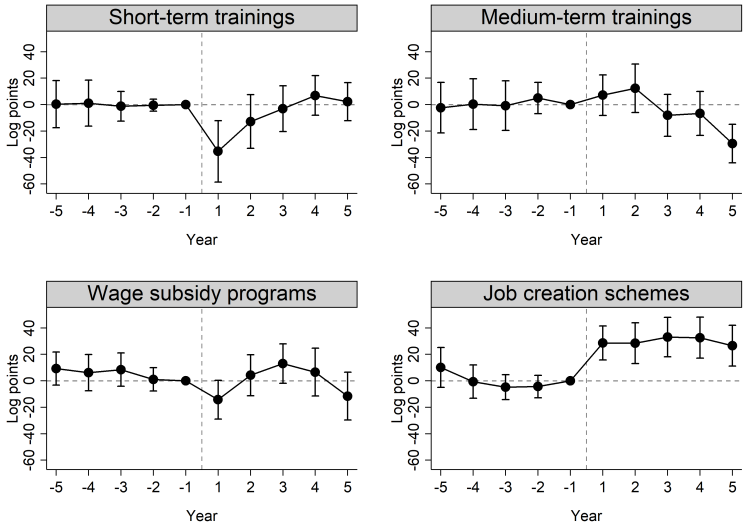
in general compared to centralized job centers. Thus, we can rule out changes in the overall use of ALMP measures accounting for the reductions in job finding. Columns 2, 3, and 4 indicate that the job center types do not differ with respect to their use of short-term training, medium-term training, and wage subsidies in a statistically significant way, although decentralized job centers tend to use these ALMP measures less intensively. Column 5 reveals that decentralized job centers sent about 30% more unemployed job seekers into job creation schemes. The lower effectiveness of this program type indicates that this policy change contributes to the observed loss in job finding. In fact, decentralization increased the average inflow rate into job creation schemes by a similar magnitude as it decreased the average aggregate job-finding rate.[10] If job seekers start a new position within the program duration in the regular labor market, they may not be counted as a job placement of an unemployed person in the official statistics. The differences in job placements between provider types then could also arise due to statistical recordings. However, according to the official statistical reports of the FEA, only about 10% of job creation scheme participants are in employment six months after leaving the program (Bundesagentur für Arbeit, 2012). Hence, job entry during program participation is an improbable explanation of the decentralization effect.

To ensure we do not overlook any underlying dynamic changes, we employ

[10] In the post-reform period, the average aggregate monthly job-finding rate of decentralized districts was 3.1% and the average monthly inflow rate into job creation schemes was 1.2%. With treatment effects of –10% and +30% respectively, the job-finding rate changed by $3.1\% \cdot \frac{-0.1}{1-0.1} = -0.34$ percentage points and the job-creation inflow rate increased by $1.2\% \cdot \frac{0.3}{1+0.3} = 0.23$ percentage points.

equation (2.2) to estimate the year-specific impacts of decentralization on the inflows into the different ALMP programs. Figure 2.4 presents the results for our four most important program types. For short-term training, medium-term training, and wage subsidies, we do not observe systematic or permanent changes that are statistically significant at the 95%-confidence level. Inflows into medium-term training appear to be slightly reduced in the long run. For job-creation schemes, in contrast, inflows increase directly after decentralization and remain at a permanently higher level.

Figure 2.4: Dynamic Treatment Effects of Decentralization on Monthly Entries Into ALMP Measures



Notes.– The figure depicts coefficients and their 95%-confidence intervals of yearly leads and lags of the decentralization indicator from a stock-flow regression of the monthly inflow into different ALMP measures as given by equation (2.2). The year 2011 is the baseline category. The regressions include a full set of dummies for job centers and months. Standard errors are clustered at the job center and the month level.

Possibly, some local authorities used the decentralization of their job centers to shift fiscal costs from their own to the federal budget. The incentive to do so is inherent to a system where the national government covers the costs of program participation and subsequent unemployment while not being able to influence the local job centers' ALMP strategy. The political component is also supported by the auxiliary analysis in Appendix A.5, showing that decentralized providers rely on job creation schemes more heavily ahead of communal elections.

2.5.3 Monitoring and Sanction Strategies

Changes in the sanction strategy of local job centers constitute another potential channel that might explain lower job finding after decentralization. Sanctions

are temporary reductions in unemployment benefits when job seekers do not comply with their job seeker obligations, such as search and meeting duties. Ample empirical evidence confirms that stricter sanction regimes and even the credible threat of being sanctioned increase the job-finding rate (see van den Berg et al., 2004; Abbring et al., 2005; Lalive et al., 2005; Boone et al., 2009) although van den Berg et al. (2014); Arni et al. (2013) imply that sanctions should not be set discouragingly high to achieve the desired outcome. We confirm this notion for our sample within a simple exploratory analysis where we regress contemporaneous job finding on previous sanction activities (see Figure A.4 in Appendix A.4). The resulting correlations show that job finding is increasing with sanctions issued in previous months. This effect is driven by the lower benefit sanctions rather than the higher sanctions which also cut accommodation costs. In sum, we expect fewer but stricter sanctions to reduce job finding.

Decentralized job centers could prefer to sanction welfare recipients less intensively as laxer enforcement may affect the job-center clients' voting behavior and increase the re-election prospects of incumbent local politicians (see Mechtel and Potrafke, 2013; Brollo et al., 2015). Centralized job centers do not encounter this incentive as the FEA is a federal institution not relying on local constituents. In addition, decentralized job centers may emphasize sanctions that lower local public expenditures due to the financing structure of welfare support in Germany. Minor non-compliance to job seeker duties will first reduce federally financed benefit payments. Severe or repeated failures to comply will lead to higher sanctions that also include reductions of the accommodation costs financed by local governments. Hence, decentralized job centers could reduce local welfare expenses by imposing stricter sanctions affecting accommodation costs more often.

We employ our baseline model from equation (2.1) to explore whether decentralization leads to changes in the monitoring strategies. Table 2.7 presents our estimates for the total number of sanctions imposed, sanctions in place, and sanction types used. There is a weak indication for a negative decentralization effect: According to column 1, the number of sanctions newly imposed in a given month decreased by about 6%. Column 2 suggests that the monthly stock of sanctions also decreased by 6%. Larger effects on the sanctions' stock than on the flow reflects that some sanctions endure several weeks, exacerbating the effect on the stock variable. We continue with the stock of sanctions because it is only possible to distinguish benefit from accommodation sanctions for this variable. The final two columns then reveal that decentralization lowered the number of benefit sanctions imposed but slightly increased the level of reductions in accommodation payments. All of these effects are not distinguishable from zero in a statistical sense. Hence, the potential conclusion that decentralized job centers imposed on average fewer sanctions overall but with a relatively higher strength among those that remained

should be treated with caution. It also rests on the assumption that job seeker compliance did not change due to the reform. Fewer but stricter sanctions might be in the interest of localized job centers for the political and fiscal considerations outlined before.

Variable	(1) New sanctions	(2) Stock of all sanctions	(3) Stock of benefit sanctions	(4) Stock of accommodation sanctions
Decentralized	−0.063	−0.066	−0.077	0.090
	(0.062)	(0.054)	(0.055)	(0.079)
R-squared	0.896	0.943	0.943	0.814
Districts	316	316	316	316
Observations	36804	36903	36903	36515

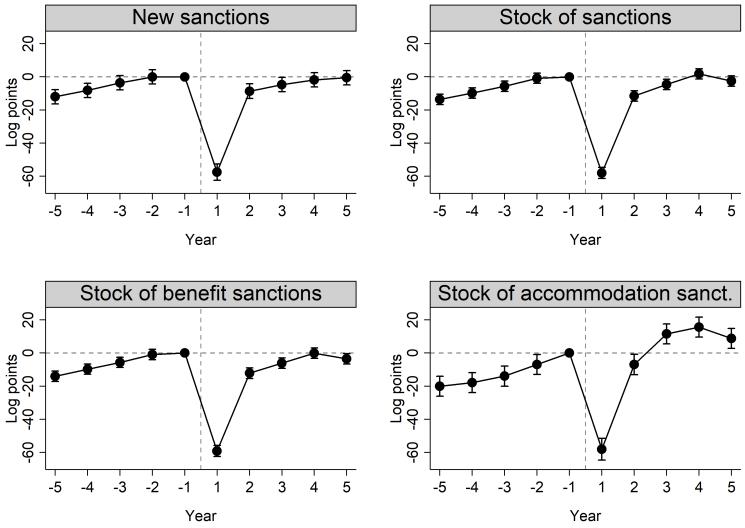
Notes.– * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. Each column presents a different estimation of equation 2.1. The dependent variables are inflows of unemployed into the respective ALMP categories. *Decentralized* is a dummy equaling 1 for districts with decentralized job centers and 0 otherwise. Regressions include the stocks and flows of unemployed and vacancies as well as a full set of dummies for job centers and months. All continuous variables in logs. Sample sizes vary due to missing observations. Standard errors given in parenthesis are clustered at the job center and the month level.

Table 2.7: Difference-in-Differences: Average Effect of Decentralization on Monthly Sanctions of Unemployed

To explore the permanency of these effects, we move again to the dynamic specification of our model as given by equation (2.2). Figure 2.5 summarizes the impact of decentralization on our four sanction outcomes. The graphs exhibit at least one significant pre-treatment effect per outcome variable, indicating a violation of the difference-in-differences assumption for sanctions. Nevertheless, the simple time pattern indicate a drastic reduction in new sanctions during the first year following the reform of about 50 log points but none in the following years. The stock of all sanctions as well as the stock of benefit sanctions followed a very similar pattern. If the graphs are interpretable at all, the results signal that decentralized job centers do not treat their clients more generously on a permanent basis. Therefore, it is unlikely that the permanently lower job finding is due to a laxer sanctioning regime. Moreover, the time pattern may help to explain the particularly pronounced drop in job finding during 2012. Caseworkers might have been busy coping with new procedures rather than actively monitoring and sanctioning job seekers at that time. Finally, there is a weak indication that decentralized job centers could have shifted their sanctions policy to include more cuts in accommodation costs of job seekers. Such a sanction strategy brings relief to local budgets but likely does not

lead to additional job placements due to its discouraging effect on job seekers.

Figure 2.5: Dynamic Treatment Effects of Decentralization on Sanctioning of Unemployed



Notes.– The figure depicts coefficients and their 95%-confidence intervals of yearly leads and lags of the decentralization indicator from a stock-flow regression on the log monthly number of new sanctions as given by equation (2.2). The year 2011 is the baseline category. The regression includes a full set of dummies for job centers and months. Standard errors are clustered at the job center and the month level.

2.5.4 Placement Strategies

Decentralizing job centers may provide gains other than higher job finding, such as improved job quality. In particular, job centers may accept a lower placement rate if they emphasize the quality rather than the quantity of their placements. In Germany, decentralized job centers may focus on stable, higher-paying placements because the districts bear the accommodation costs for households on welfare, irrespective of the employment status. In contrast, centralized job centers have an incentive to focus on the number of placements regardless of job quality as any person exiting unemployment reduces FEA expenditures.

We assess the effect of decentralization on the placements' quality using outflows from welfare rather than from unemployment. The welfare data consider all people on welfare of which only about half are registered as unemployed. The remaining welfare recipients are mainly ALMP participants, employed but earning low incomes or unable to work due to familial or health reasons. Total outflows from welfare are about two to three times larger than flows from unemployment into jobs. People will exit welfare if their household income exceeds a subsistence

threshold that varies according to household size and local costs of living. They may also exit at the start of retirement. Therefore, our identification strategy rests on the assumption that decentralization affected welfare outflows only via job placements and that other components of welfare outflows remained unaffected by this reform or simultaneous unobserved shocks.

Table 2.8 presents our estimation results. In all columns, we re-estimate equation (2.1) using vacancies, welfare stocks, and their inflows as control variables. We again also include a full set of job center as well as month fixed effects. In column 1, we focus on the total outflow out of welfare as a rough indicator for reemployment wages. We do not observe a statistically significant effect of decentralization, implying that decentralized job centers do not achieve more high-paying placements than their centralized counterparts. However, they also do not perform significantly worse. Apparently, the lower unemployment outflows observed above do not translate into fewer welfare outflows after decentralization. Two explanations can reconcile these findings. First, unemployment outflows are too small compared to welfare outflows such that the negative effect on the former does not carry over to a negative effect on the latter. Second, decentralized job centers were reluctant to place their clients into low wage jobs while their placement efficiency for higher paying jobs was not affected by decentralization. Further research using more detailed data will be necessary to disentangle these two explanations.

Variable	(1) Outflows out of welfare	(2) Permanent outflows out of welfare	(3) Share of permanent outflows
Decentralized	−0.007	0.002	0.604
	(0.015)	(0.014)	(0.705)
R-squared	0.981	0.975	0.438
Districts	334	334	334
Observations	38718	38718	38718

Notes.– * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. *Decentralized* is a dummy equaling 1 for districts with decentralized job centers and 0 otherwise. Standard errors given in parentheses are clustered at the job center and the month level. Regressions include the stocks and flows of unemployed and vacancies as well as a full set of dummies for job centers and months. Differing sample sizes are due to missing data in few observations.

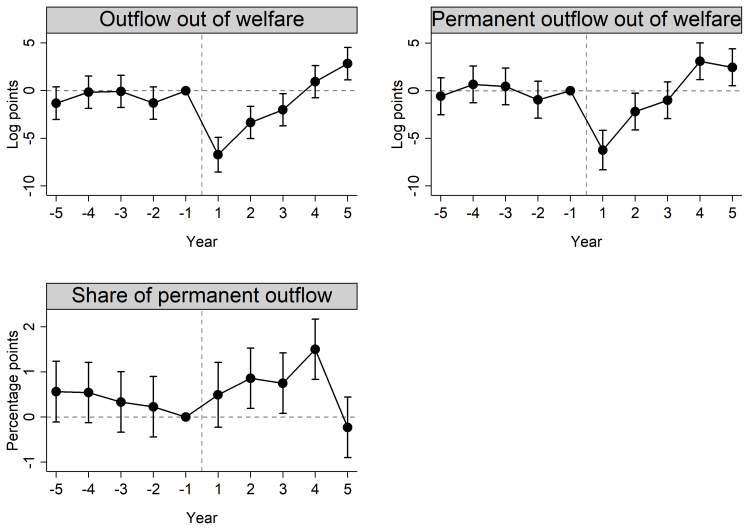
Table 2.8: Difference-in-Differences: Average Effect of Decentralization on the Composition of Monthly Outflows and Unemployment Stocks

In column 2, we narrow our analysis to welfare outflows without returns into welfare during the next three months ('permanent outflows'). We consider this as

a rough measure of the placements' stability. Again, we do not find a statistically significant impact of decentralization. When using an alternative outcome measure, the share of permanent placements in all outflows from welfare in column 3, we observe a small positive, but not statistically significant impact of decentralization.

Figure 2.6 presents the respective dynamic treatment effects. These vary strongly over time, with a drop in total welfare outflows around the decentralization period and a trend towards more total and permanent welfare outflows in the medium run. Yet, the treatment effects are often at the margin of statistical significance. The share of permanent outflows increases up to post-reform year four before returning to the baseline level in the final period of the sample window. We conclude that decentralization had no overall impact on the placement quality in the five years following the reform, although positive tendencies exist especially in later periods. Increasing welfare outflows may take a longer time to materialize and require a long-term analysis.

Figure 2.6: Dynamic Treatment Effects of Decentralization on Monthly Welfare Outflows



Notes.– The figure depicts coefficients and their 95%-confidence intervals of yearly leads and lags of the decentralization indicator from a stock-flow regression of the monthly outflow out of welfare or the share of permanent outflows as given by equation (2.2). The year 2011 is the baseline category. The regressions include a full set of dummies for job centers and months. Standard errors are clustered at the job center and the month level.

2.5.5 Further Considerations

Current literature is increasingly emphasizing the importance of caseworker characteristics in the job matching process (see, for instance, Behncke et al., 2010;

Hainmueller et al., 2016). Job placements will suffer from decentralization if decentralized job centers reduce the number of caseworkers or replace experienced ones with less qualified employees. In our example, however, this is not the case. Due to the law regulating decentralization reform, about 95% of the administrative and caseworker staff in the decentralized job centers continued to work for the communal job centers after their reform. The law (§6c SGBII) also prescribed that employees and civil services should retain their prior wages and hierarchy levels. A report for the German parliament confirmed that districts complied with the provisions of the law. Consequently, changes in the job-center personnel cannot explain permanently reduced job finding.

Moreover, differences in the controlling systems possibly contribute to lower job finding through decentralized job centers. As described in Section 2.2, centralized job centers are under the technical supervision of the FEA while decentralized job centers are not. The FEA imposes a very rigorous target control system on centralized job centers that include target agreements, performance dialogs, ranking comparisons, and strict monitoring by a federal institution (Vorstand der Bundesagentur für Arbeit, 2014). Decentralized job centers have to report to state ministries but otherwise remain independent. They are members of a voluntary benchmarking program organized by the Federation of German Cities and Communes. Interview partners from ministries and job centers suggest that the FEA controlling system has tighter requirements with a stronger emphasis on quantitative outflow measures. Hence, it may partially explain why centralized job centers generate more job placements.

2.6 Sensitivity Analyses

The results presented thus far suggest that decentralization decreased job finding while increasing the inflows into job creation schemes. We now assess the validity of these inferences in detail. There are three major concerns. First, the common trends assumption might be invalid due to the state-quota system inducing a selection problem or due to unobserved labor market shocks. Second, the SUTVA might be violated if labor markets extend beyond district borders and spatial spillovers between treated and non-treated districts arise. Third, our findings might rely on overly restrictive functional form assumptions and other model specifications. In the following paragraphs, we provide a battery of analyses to address each of these concerns. We will focus on our main outcome, the outflow of unemployed into jobs, and provide results for the other main outcomes in Appendix A.6.4 (Tables A.6, A.7, and A.8).

2.6.1 Selection and Unobserved Shocks

Table 2.9 summarizes the results of several checks regarding selection and the common trend assumption. The first column analyzes the districts' decision to apply for decentralization. Districts might have based this decision on some time-varying characteristics that are unobserved in our data. If applicants and non-applicants differ significantly from each other with respect to such characteristics, our decentralization estimates are biased. We control for this bias using two alternative specifications. First, we restrict our control group to the non-successful applicants and re-estimate equation (2.1). If this restriction drives our decentralization estimate down to zero, our main specification has estimated an application rather than a decentralization effect. However, column 1 of Table 2.9 demonstrates that our estimated decentralization effect on job finding is still –9% using the restricted control group. As this result is very similar to our initial estimate, we take this analysis as initial evidence that applicants and non-applicants do not differ systematically from each other.

Variable	(1) Denied applicants as only controls	(2) Denied applicants as treated	(3) Over-subscription subsample	(4) Conditional DiD	(5) Triple differences
Decentralized	−0.087 ***	−0.006	−0.095 ***	−0.095 ***	−0.071 ***
	(0.025)	(0.017)	(0.021)	(0.020)	(0.019)
R-squared	0.943	0.960	0.952	0.952	0.975
Districts	75	294	309	330	334
Observations	8722	34395	36093	38550	78096

Notes.– * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. *Decentralized* is a dummy equaling 1 for districts with decentralized job centers and 0 otherwise. Standard errors given in parentheses are clustered at the job center and the month level. Welfare data from November and December 2011 excluded due to errors. Regressions include the stocks and flows of welfare recipients and vacancies as well as a full set of dummies for job centers and months.

Table 2.9: Effect of Decentralization on Log Monthly Flows Into Jobs for Different Control and Treatment Groups

As an alternative control for self-selection at the district level, we define the unsuccessful applicants as a placebo treatment group and compare their outcomes to the districts that did not apply for decentralization, i.e. we estimate the effect of being interested but not actually being decentralized. If this estimate is statistically significant, applicants likely differ from non-applicants. As column 2 of Table 2.9 presents, the applicant status has no such effect on job finding. Thus, we conclude

that applicants and non-applicants do not differ with respect to relevant unobserved, time-varying characteristics.

Successful and non-successful applicants will differ from each other if state governments in the state-quota process successfully chose those applicants for decentralization that were most likely to reap the greatest benefit from decentralization. Our estimated negative main decentralization effect would then be biased upwards and the true effect was even more negative. We assess this kind of selection by restricting our sample to states where the number of applicants exceeded the state quota ('oversubscription') and governments had an actual choice among applicants. Selection would be an issue if estimating equation (2.1) results in less drastic reductions using the 'oversubscription' subsample than when using the baseline sample. Column 3 of Table 2.9 shows that the decentralization effect for the 'oversubscription' subsample is incredibly similar to our baseline estimate. Hence, selection into decentralization at the state-level is also unlikely.

We now ask whether job centers of the treatment and the control group have experienced different labor market trends for reasons unrelated to the formal selection process. If observable characteristics influence the unobserved trends, reweighing our observations with regard to these characteristics should reinforce the validity of common trends assumption and should affect our baseline decentralization estimates significantly. Therefore, we employ a variant of the conditional difference-in-differences estimator (see Heckman et al., 1997, 1998, and Appendix A.6.1 for details). As column 4 of Table 2.9 indicates, our estimates of the decentralization effect on job finding hardly change due to the balancing. This implies that labor market trends of decentralized and centralized job centers did not depend on observable characteristics.

Finally, districts from the treatment and the control group could have experienced systematically different unobserved labor market shocks that affect our estimates. The widespread geographical distribution of treated districts makes such an event unlikely. Here it is important to note that the particular German institutional setup allows for an explicit assessment. We exploit data available due to job seekers usually receiving public employment services from local employment offices and not from the job centers during their first 12 months of unemployment.[11] These local but federal employment offices are centrally organized throughout Germany which were neither directly nor indirectly affected by the decentralization of job centers. However, district-specific labor market shocks and trends should affect the unemployed registered at local employment offices and job seekers registered at job centers alike. We use the unemployed registered at the local employment offices in the same district as an additional comparison group to control for time-varying district-specific shocks in a triple differences estimation (see, for instance,

[11] This is because unemployed receive unemployment insurance benefits during this period.

Gruber, 1994). As shown by column 5 in Table 2.9, the decentralization effect on unemployment outflows from job centers is again estimated to be about –10%. This result affirms that our preferred specification is not biased by unobserved district-specific shocks, and that the common trend assumption is likely to hold.

2.6.2 Spatial Spillovers

We now address the second major concern to validity, potential spillovers among districts. In particular, we worry about indirect treatment effects on non-decentralized job centers and labor market regions extending beyond district borders. Table 2.10 summarizes the results for this analysis.

Variable	(1) Controls without non-treated neighbors	(2) Spatial lag in X	(3) X measured at commuting zone level
Decentralized	−0.093 ***	−0.100 ***	−0.133 ***
	(0.022)	(0.021)	(0.023)
R-squared	0.954	0.958	0.956
Districts	222	334	334
Observations	25915	39018	39018

Notes.– * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. Each column presents a different estimation of equation 2.1. The outcome variable is the monthly log outflow out of unemployment into jobs. *Decentralized* is a dummy equaling 1 for districts with decentralized job centers and 0 otherwise. Standard errors given in parentheses are clustered at the job center and the month level. Regressions include the stocks and flows of unemployed and vacancies as well as a full set of dummies for job centers and months.

Table 2.10: Assessing SUTVA and Spatial Effects: Effect of Decentralization on Monthly Log Flows Into Jobs for Different Model Specifications

Indirect treatment effects arise if job finding in decentralized job centers declines and if centralized job centers in neighboring districts advise their clients on the 'additional' unmatched vacancies. This would increase job finding in centralized districts bordering a treated region. Such spillovers would dilute the control group, violate the SUTVA, and exaggerate our estimate of the true reduction in job finding. To examine this problem, we drop all units from the control group that border districts with decentralized job centers. If any spillover effects dilute our baseline specification, this change to the control group should reduce the size of the estimated treatment effects. However, column 1 of Table 2.10 confirms our baseline estimate. Combining these results with our analysis on geographical lock-in in section 2.5.1, we conclude that decentralization did not generate spillover effects on non-decentralized districts.

In spite of this finding, there might be more complex spatial patterns with spillovers reaching beyond direct neighbors. For instance, job search competition is larger if unemployment is large and vacancies are scarce in nearby districts. To capture such effects, we estimate a standard spatial lag in X model (see LeSage and Pace, 2009). We add spatial lags for each explanatory variable in our baseline regression using row-normalized inverse distances between districts as respective spatial weights. As column 2 of Table 2.10 shows, the inclusion of spatially lagged covariates does not alter our results. To confirm this finding, we aggregate the stocks and inflows of unemployed and vacancies on the commuting-zone level based on the commuting zones definition by Kropp and Schwengler (2016). Column 3 presents our estimation of equation (2.1) employing the commuting-zone variables. The result reveals that our decentralization effect remains very similar to previous estimates, albeit with a slightly higher magnitude of about –12%. In summary, none of the three spatial approaches used suggests that geographic spillovers invalidate our main findings.

2.6.3 Model Misspecification

Finally, we analyze whether our model imposes improper functional form assumptions and whether standard errors are calculated correctly. To relax the functional form assumption, we run a synthetic control approach following Abadie and Gardeazabal (2003) and Abadie et al. (2010). This method is purely data-driven and non-parametric. Nevertheless, its results, described in Appendix A.6.2, are highly similar to those derived from our stock-flow model in equation (2.1). We conclude that our model does not impose improper functional form assumptions.

Next, we examine whether our standard errors are correctly sized and do not overstate the significance of our findings. Serial correlation in particular, which we deal with by two-way clustering standard errors at the district and month-level, is a frequent concern in difference-in-differences studies (Bertrand et al., 2004). Following Huber et al. (2013), we run an empirical Monte-Carlo simulation on our subsample of non-treated districts. In each replication, we randomly assign a placebo treatment status to 41 districts and then estimate the effect of the placebo treatment as in our main model. With 5,000 replications, we find significant pseudo-decentralization effects at the 5% level in less than 5.9% of all cases. Furthermore, we inspect the distribution of the resulting t-statistics for the decentralization coefficient to confirm it follows a normal distribution (Figure A.6 in Appendix A.6). Both checks yield adequate results and ensure that the size of our standard errors is correct.

2.7 Conclusion

Few studies have examined the impact of decentralizing public employment services although numerous countries have implemented such reforms. In this paper, we provide the first comprehensive analysis of public employment services under decentralization and their effect on job finding and labor market policies. Exploiting a unique German policy experiment that transferred 41 federally-managed job centers to the district level, we estimate that job-center decentralization reduced job finding by approximately 10% within five years. Estimates from dynamic models point to the existence of a reform transition period lasting for about one year. Nevertheless, efficiency losses are still sizable even five years after decentralization. We uncover that decentralization leads to a significant increase of inflows into job creation schemes while leaving overall ALMP participation unchanged. Moreover, decentralization temporarily reduced benefit sanctions in the first year after decentralization, likely reflecting a transitional process. In contrast, we do not find evidence for higher quality placements or increased geographical lock-in of job seekers. Finally, we can rule out a difference between the two provider types driven by caseworker quality or quantity.

The persistent drop in job finding combined with the increased use of job creation schemes indicates that local politicians possibly utilized decentralization to shift fiscal costs from their own to the national budget. Local administrations have tangible financial benefits from job creation scheme participants in the form of public goods, while the federal government primarily bears the costs of program participation and subsequent unemployment.

The decentralization reform had substantial impacts on public budgets. Assuming outflows into non- or self-employment remained unaffected, average unemployment duration in decentralizing job centers increased by about 3 months. Since benefit and accommodation payments amount to about 820 euros per unemployed person per month (see Weber et al., 2014, p. 4), these figures imply additional costs of about 2,500 euros per unemployed. On average, 27,000 persons register as new unemployed at the 41 decentralized job centers each year, not accounting for re-entries after ALMP measures or very short employment spells (see Hofmann and Stephan, 2016). Thus, a conservative estimate is that the 2012 decentralization caused additional annual costs of at least 66 million euros. Further fiscal burdens arise from prolonged job counseling, additional ALMP participations and foregone tax revenues.

Our findings are informative for policy makers considering to reform and decentralize public employment services. Canada, Denmark, Italy, and other countries have undergone significant decentralizations in the past but cannot evaluate the impact of their reforms because they lack a proper treatment-control-group design. Other countries, including Germany, have been discussing whether to (further)

decentralize their public employment services. Our findings imply that decentralized job centers may fail to internalize the effects of their strategies on total public budgets and individual reemployment rates. More generally, they strongly suggest the importance of carefully studying the incentive effects arising from decentralization, as ill-designed institutional structures may significantly reduce the job centers' matching efficiency.

Therefore, this analysis should serve as a starting point for further research distinguishing the impacts of decentralization under alternative financing rules and division of competences. Additional research is also necessary to study the internal structures and strategies adopted by centralized and decentralized job centers in more detail. Moreover, the interaction of localized provision modes with the political sphere is clearly under-explored. Finally, long-term effects extending beyond the temporal constraints of this paper, as well as alternative outcome measures, such as re-employment wages and match durations, will help to understand the consequences of decentralization. The decentralization of public employment services remains a crucial topic for future research.

3 Fueling Fiscal Interactions: Commodity Price Shocks and Local Government Spending in Colombia

3.1 Introduction

In the past four decades, decentralization has been a focal point of policy reform in many developing countries, including South American nations. Policy advisors such as the World Bank and the OECD have advocated the delegation of more fiscal autonomy concerning both public expenditures and revenues away from the central government to local governments, including municipalities. More autonomy of local governments opens up the possibility of local fiscal interactions.

To fully understand the consequences of decentralization, reliable estimates of the extent of strategic fiscal interactions of local governments are crucial, because strong interactions may imply externalities and therefore inefficiencies (e.g., Caldeira et al., 2014). On the revenue side, tax competition has received considerable interest and raised concerns that uncoordinated fiscal autonomy may lead to a race to the bottom of tax revenues. Far less is known about local fiscal interactions on the expenditure side, especially in a developing country context.[1] Strategic competition in expenditures may differ from strategic tax competition (e.g., Wildasin, 1988), so existing empirical evidence on tax interactions does not necessarily carry over to expenditure interactions. Despite the scarcity of evidence, local expenditure spillovers are frequently discussed in policy debates. For example, local policymakers are often concerned that too generous local welfare spending will draw in welfare migrants from surrounding regions (see Figlio et al. (1999) for an analysis of welfare competition at the level of US states). Neighboring regions may then save public funds at the expense of the local taxpayers of the focal jurisdiction. Such considerations may lead to an underprovision of certain public goods if local governments are responsible.

Expenditure interactions may also arise if local policymakers engage in yardstick competition and mimicking of peers.[2] Understanding the way local governments

[1] As an illustration, Google Scholar finds 30,000 papers for "tax competition" but only around 600 results each for "expenditure competition" or "spending competition" (September 2016).

[2] The website of the mid-sized municipality Fresno in Colombia suggests that policymakers

interact in terms of public expenditures is key for implementing development policies which aim to improve local public spending. If local policymakers mimick their neighbors, it might be effective to roll out an innovative budget composition in a local jurisdiction that is used as a benchmark, i.e. that is central in the spillover network. Then the expectation is that the policy spreads out to other regions. If public expenditures are strategic complements, decentralized foreign aid to one municipality may entail fiscal efforts in connected regions and trigger a multiplier effect (Caldeira et al., 2014; Glaeser et al., 2003). Public expenditure interactions may be very different for different types of expenditures. Therefore, it is important to distinguish between various categories of local expenditures.

In this paper, we analyze spatial interactions of different types of local public expenditures among municipalities in Colombia. Our data covers the universe of the more than 1000 Colombian municipalities over eleven years from 2000 to 2010 and distinguishes between the eleven most important expenditure categories. Studying Colombia is highly informative because the country has undergone fiscal decentralization reforms similar to those in many countries in South America and the developing world. Colombia is also similar to many Latin American and other developing countries in that they collect substantial royalties for the extraction of natural resources and partly allocate them to subnational governments.

The methodological challenge in the literature on spatial spillovers in general and local fiscal interactions in particular is the identification of causal spatial interaction effects. These must be separated from spatial autocorrelation that is due to spatially clustered unobservable factors (Manski, 1993; Gibbons and Overman, 2012; Revelli, 2015). For identification, we exploit the fact that Colombian municipalities receive royalties that depend on revenues from local oil extraction by private companies. Higher local oil revenues thus relax the budget constraint of a municipality and allow an increase in local public spending. To identify expenditure spillovers, we use exogenous variation in the exposure of municipalities to changing world market prices for oil due to their endowments with oil resources (as measured before our time period of analysis). With our spatial panel instrumental variable (IV) estimator, we control for municipality fixed effects as well as department-specific time fixed effects. This accounts for any cross-sectional differences between municipalities with and without oil resources as well as regional business cycle effects while leaving changes in world market prices of oil for exposed municipalities for identification. World market prices for oil are arguably exogenous for Colombia as a relatively small oil extracting country, which is not a member of OPEC, and certainly for Colombian municipalities.

compete locally: "Municipalities [...] must develop integral systems to generate resources as well as social and economic development based on three basic variables: competitiveness, productivity and employment. Competitiveness is understood as the capacity of the municipality to differentiate itself from the surrounding municipalities to attract investments." http://fresnoposible.fresnodigital.info/?page_id=56 (accessed 09/10/2016)

Our results indicate that spatial interactions of total local public expenditures as well as local spending in almost all categories are small and not significantly different from zero when identifying the spillover effects based on quasi-experimental variation. The only exception is public spending for sport and recreation, where we detect significant spatial interactions that can be interpreted as causal. In contrast, when we use a spatial panel estimator without our additional source of exogenous variation, as done in most of the literature, the results suggest large and significant spatial autocorrelation in total local expenditures and almost all spending categories, even when controlling for municipality and time fixed effects. However, most of these effects are spurious, as we show by comparison to the consistent estimation based on the quasi-experimental instrument. Thus, the main methodological insight from our analysis is that it is crucial to use additional sources of exogenous variation for the identification of spatial fiscal interaction effects in a quasi-experimental approach.[3]

As spatial interactions are insignificant for total local public spending and most expenditure types, the main policy insight from our paper is that policymakers do not need to be overly concerned about a race to the bottom regarding local public expenditures when pursuing decentralization reform in a developing country. On the other hand, one should also not expect that innovative local expenditure policies spread out through mimicking in substantial ways. A potential caveat is that transfers and royalties that municipalities in Colombia receive are partly earmarked for spending in specific categories. This might limit the scope for expenditure interactions in Colombia, although we discuss below that Colombian local policymakers seem to have significant leeway.

Our approach and results contribute to a small body of emerging literature that employs quasi-experimental sources of variation for identification in the context of spatial fiscal interactions. Concerning expenditure spillovers, Baicker (2005) uses variation in federally-mandated increases in Medicare spending at the state level in the United States, and Isen (2014) employs referendum decisions in counties and municipalities in Ohio. While the former author reports considerable spatial spillovers, the latter does not find any significant interaction effects using his identification strategy. We largely confirm the last result in a developing country context and based on a very different source of exogenous variation, but also show that causal fiscal interaction effects can be identified for the category of sport and recreation.

In the context of tax competition, the literature using quasi-experimental approaches has developed more rapidly. Lyytikäinen (2012) uses changes in minimum property tax rates in Finland for identification, and Baskaran (2014) a fiscal equalization reform in Germany. Both authors report that seemingly large tax interactions

[3]Spatial discontinuity designs also fulfill this request.

become insignificant when using quasi-experimental variation for identification, similar to the result of Isen (2014) concerning public expenditures. Other papers that identify fiscal effects at borders of higher level jurisdictions or language regions report significant strategic tax interactions (Eugster and Parchet, 2013; Parchet, 2014; Agrawal, 2015; Agrawal, 2016). It remains unclear whether the inconclusiveness in the spatial tax interactions literature using additional exogenous variation is due to the different institutional settings analyzed or whether it can be explained by differences between the quasi-experimental and spatial discontinuity approaches. Revelli (2015) mentions that by dropping municipalities with low tax rates from his analysis, Lyytikäinen (2012) may have excluded the municipalities most likely to respond to tax competition pressures, which could explain his finding of no interactions.[4]

Most of the literature on spatial fiscal interactions does not use quasi-experimental variation. While that literature is fairly large in the context of tax competition, not many papers exist on public expenditure spillovers. Case et al. (1993) provide an analysis at the level of US states. Analyzing spillovers at the municipality-level provides a much larger policy variation compared to the state- or department-level mostly employed in the literature (e.g., Baicker, 2005). Moreover, most papers only analyze few expenditure categories or solely focus on single budgetary items such as health expenditures (Moscone et al., 2007). Borck et al. (2007) are among the few authors who use municipality data and distinguish between various spending categories, although only cross-sectionally.

The literature on fiscal interactions largely ignores developing countries since large and complete fiscal policy datasets rarely exist in these regions. It is important to investigate developing countries separately because they are in the focus of decentralization reform efforts. Additionally, they differ from developed countries in their forms of decentralization and institutional as well as budgetary constraints. Akin et al. (2005) investigate health care budgets in less than 30 districts in Uganda and Arze del Granado et al. (2008) local public spending based on cross-sectional data from Indonesia. Agostini et al. (2016) and Yu et al. (2016) analyze spatial spending interactions in China. Caldeira et al. (2014) use a panel of 77 communes in Benin

[4]To further investigate whether the identification approach influences results within the same country and institutional setting, it would be interesting to compare our approach using quasi-experimental variation between municipalities with an approach using spatial discontinuities at the borders of higher-level jurisdictions. In principle, Colombia could be suitable for such a comparison as departments also receive royalties from the exploitation of natural resources, potentially providing a spatial discontinuity setting. However, institutional details complicate such an analysis because departments near oil producing departments may also receive a share of the royalties ("*escalonamiento*"). At the municipal level, a similar mechanism rarely applies and if so, it affects all municipalities within the same department. In our approach we can account for this by using a control variable (transfers) and department-time fixed effects. Furthermore, departments have much less autonomy than municipalities regarding taxes and public expenditures in Colombia (Bird, 2012). Given the institutional and data issues, a spatial discontinuity approach is not within the scope of this paper, but might be an interesting avenue for future research.

and report strategic complementarity of local public spending. The significant fiscal externalities found in Caldeira et al. (2014) question the efficiency of decentralization reform in developing countries. In contrast to our paper, the existing literature in the developing country context does not use quasi-experimental variation for identification and may therefore overestimate expenditure interactions.[5]

Our paper is also related to studies using an identification strategy similar to ours based on Colombian data. Martinez-Vazquez et al. (2017) shows that local governments in Colombia perform better in providing public goods if more revenues come from their own taxes rather than oil royalties. He exploits exogenous variation in oil revenues for identification. Dube and Vargas (2013) investigate effects of income on civil conflict in Colombia using commodity price shocks.

This paper proceeds as follows: The next section reviews the theoretical literature, while Section 3.3 introduces the relevant Colombian context. Sections 3.4 and 3.5 present the data and empirical strategy before we discuss our results in Section 3.6. Section 3.7 concludes the analysis.

3.2 Theory of Expenditure Interactions

There are at least four different reasons why expenditures might be linked among municipalities. First, yardstick competition (Shleifer, 1985; Besley and Case, 1995) may induce local politicians to mimic their neighbors' policies. This is because voters can assess the quality of their incumbent politician only by observing policies in neighboring jurisdictions. The theory was originally developed to explain spillovers in local tax-setting and has been subject to vast empirical tests (see Allers and Elhorst (2005) for an overview). The mechanism may also apply to local public spending and result in positive interactions among local expenditures. Yardstick competition might be particularly relevant in developing countries with elected local governments like Colombia because information problems are likely to be comparably severe.[6]

Second, expenditures may be linked due to competition for attracting a mobile tax base as suggested by Tiebout-type models (Tiebout, 1956). For instance, municipalities could compete for company settlements or highly-skilled workers (Borck et al., 2007) by investing in their communication infrastructure or education system. In this case, the interjurisdictional spending is positively correlated. The classic model of fiscal spillovers by Zodrow and Mieszkowski (1986) shows how local governments competing for mobile capital will underinvest in public services. Keen and Marchand (1997) argue that non-coordinated spending of local governments

[5] Yu et al. (2016) find spatial interactions within, but not across Chinese provinces, which supports their conclusion that Chinese local leaders engage in tournament competition.

[6] Faguet and Sánchez (2013) conclude that decentralization reform has made Colombian mayors more accountable.

under tax competition may be downward biased relative to the efficient level. They also posit that the underprovision of public consumption-oriented investment compared to public production inputs is relatively more pronounced. This is because capital is assumed to be more mobile than households and thus receives a more favorable treatment by the government.

A third explanation for expenditure interactions is the externality-producing nature of public investments (Case et al., 1993), which may either be substitutes or complements to spendings in other regions. The sign of the correlation implied by this channel is ambiguous: On the one hand, a neighboring hospital might be sufficient to fulfill the regional demand for health services, making additional investment in another hospital obsolete. Investments then are substitutes and expected to be negatively correlated among neighbors. On the other hand, if a road construction project is supposed to link two municipalities, public local investments are likely to be complementary and positive expenditure interactions are expected. Hence, investments likely differ in their external effects, which is why we study different spending categories separately.

A fourth potential mechanism that could lead to horizontal interactions between municipalities is competition for bailouts (Baskaran, 2012). Municipal governments may believe that a higher level government authority might bail them out in case of an imminent insolvency, but that a budget constraint at this higher level might limit the scope for bailouts. A municipality's chances of a bailout and thus incentives to spend and incur debt then depend on other municipalities' expenditure and debt levels, which would lead to horizontal interactions. Municipalities in Colombia are allowed to incur debt, and this led to municipal debt problems in the 1990s. However, reforms in 1997 and 2000 introduced strong borrowing and bailout restrictions for subnational governments, which led to a sizable municipal debt reduction (Villar et al., 2013). Therefore, bailout expectations may not be as relevant in Colombia in our period of analysis as they might have been before.

The first two mechanisms discussed above would lead to positive interactions in local expenditures. The third and forth mechanisms would lead to nonzero interactions, but the sign is ambiguous in these cases. Using data from German federal states, Baskaran (2012) estimates that bailout competition leads to positive interactions as well.

Fiscal competition in developing countries may differ from the case of developed economies usually studied. Caldeira et al. (2014) emphasize that poor municipalities in developing countries may be restricted in their spending choices. They develop a model of expenditure competition with a constrained Nash equilibrium. One implication of the model is that there may be *no* strategic interactions despite positive externalities if the level of fiscal resources is insufficient. However, for the case of Benin, the authors empirically find significant positive strategic inter-

actions. Colombia is a developing country (GDP per capita: US$6,056), but less constrained than Benin (GDP per capita: US$762)[7], so we expect to find nonzero fiscal interactions in Colombia as well.

Spatial expenditure reactions may depend on the source of funds used to finance local spending. In Colombia, transfers municipalities receive from the central government as well as royalties from the extraction of natural resources are partially earmarked for spending in specific categories such as education, as detailed in the next section. Basic theory suggests that earmarking should not have practical relevance, because local governments can reallocate other funds and adapt other tax and spending decisions to offset the intended allocation. Thus, a block grant nominally earmarked for a certain category should have the same effect as a general increase in local taxpayers' income of the same amount (see Smart, 2007, for an overview on the incentive effects of grants). However, the empirical literature provides evidence that block grants are disproportionately used for public spending rather than for tax cuts and also tend to stick to the spending category they are intended for, a phenomenon known as the flypaper effect (e.g. Hines and Thaler, 1995; Inman, 2008). If earmarking in practice limits the freedom of municipalities in their spending decisions, this may attenuate the scope for expenditure interactions between municipalities in Colombia.

In summary, from the theory we expect to find nonzero and most likely positive expenditure interactions between municipalities in Colombia, although interactions might be smaller than in fully developed economies or in settings with fewer institutional constraints.

3.3 Fiscal Policy in Colombian Municipalities

Colombia has been a politically, administratively and fiscally centralized country throughout most of its history. However, as many other Latin American countries, Colombia phased in important policies towards decentralization starting from the mid 1980s (Acosta and Bird, 2005; Alesina et al., 2005; Chaparro et al., 2005). The goal of the reforms was to delegate more functions to lower tiers of government, namely Colombia's 32 departments and more than 1000 municipalities. To this end, in 1991, a new constitution introduced a number of provisions regarding the delegation of administrative and fiscal duties from the central to the lower tiers of government. Among them were increases in the amount of transfers to local and regional governments and rules on how to spend these resources. In 2001, a unified system to transfer the resources to the lower tiers of government called *General System of Participations* (SGP by its initials in Spanish) was created.

[7] Figures for 2015, given in current USD and retrieved from World Bank (2017).

In fiscal terms, these reforms have made the municipal sector the most important subnational level of government in Colombia (Acosta and Bird, 2005). Municipalities possess three main sources of income. First, municipalities obtain the aforementioned transfers from the central government. Second, municipalities have their own revenues coming from local taxes, mainly the property tax and the ICA tax (a tax on industrial and commercial activities). Third, municipalities receive royalties from the exploitation of natural resources. Each source accounts roughly for 50%, 30%, and 5% of the total amount received by the municipalities respectively (Bonet et al., 2014). The remaining revenue comes from non-tax income such as capital dividends, leased property or fees from construction permits.

Royalties from oil extraction are particularly relevant for this analysis as we use them as our source of exogenous variation. In Colombia, private companies extract oil and pay a fixed share of their oil revenues (in Colombian Pesos) as royalties according to this formula:

$$Royalty_{i,t} = output_{i,t} \times world\ market\ price_t \times exchange\ rate_t \times fixed\ royalty\ rate \quad (3.1)$$

The municipalities where the oil was extracted (as well as oil ports) receive a share of these royalties according to a fixed schedule defined by law with rates decreasing in local oil output. For those municipalities extracting oil or with an oil port, royalties are very important, accounting on average for 23% of their total revenues.[8] Oil is the most important natural resource in Colombia and accounts for 69% of total royalties, followed by coal with 23% (Martinez-Vazquez et al., 2017).

There are several rules on how to spend the revenues from the different sources of income. According to a law introduced in 2001[9], 4% of the federal SGP transfers were to be used for special purposes like food programs in schools or indigenous reservations. Of the remaining 96%, 58.5% had to be used for education, 24.5% for health, and 17% for a general purpose category, including water and sanitation, housing, and agricultural investments, among other items. In 2007 a new change in the legislation[10] gave continuity to the transfers' system, marginally modified the growth rate of the transfers to each category, separated water and sanitation from the general purpose category and assigned it 5.4% (leaving 11.6% for general purposes). After the allocation of the transfers to the different categories, the ministries in charge distribute them between the municipalities. In terms of own revenue, municipalities have almost complete freedom to spend the money they

[8] We use the term royalties to refer to direct royalties, i.e. royalties that directly go to the oil extracting municipalities. An oil extracting municipality with average (or median) oil output receives 32% of the total local oil royalties and the department receives 52% in this case. Another 8% are allocated to municipalities with oil harbors. The National Royalty Fund distributes the remaining oil royalties (indirect royalties) to municipalities that apply for specific investment projects and get approved (Bird, 2012). Our control variable "transfers" also accounts for these indirect royalties.

[9] Law 715, 2001

[10] Law 1176, 2007

collect in taxes. The only provision in this regard is a minor mandatory contribution to the regional environmental agency.

Concerning expenditure of direct royalties from the exploitation of natural resources, legislation mandates that municipalities spend at least 75% in the achievement of coverage goals for health and child mortality, education, and water and sanitation.[11] In the following, we refer to the corresponding spending categories health, education and water as earmarked categories. If all the goals are achieved, local governments can use all the royalty revenues as desired.

The rules on how certain revenue types have to be spent could restrict spending decisions and limit the potential for expenditures interaction across municipalities (Bird and Smart, 2002). However, the evidence shows that local governments in Colombia possess considerable leeway in the composition of their spending. Drazen and Eslava (2010) demonstrate that incumbents increase expenditure in public goods that are more visible to citizens in a bid to increase their vote shares before elections. According to Sánchez et al. (2004, p. 3), municipalities may relocate their own-source revenues to circumvent conditionalities and conclude that "there is considerably more fungibility in grants than a strict reading of the law would suggest". Thus, municipalities can direct some of their investments according to their own desires, leaving the door open for expenditure interactions between local governments.

Nevertheless, according to Perotti (2005), the central government's attempt to force municipalities to spend disproportionately more on categories such as health and education has prevented local governments from using resources in other areas like social programs that might have had a larger impact on local poverty reduction and therefore on the welfare of individuals. By identifying categories of public spending where local governments in Colombia compete, our paper informs policymakers which spending areas local governments are focusing on, in turn revealing local electorates' preferences.

The case of Colombia is informative in a more general context because many developing countries, especially in Latin America, implement similar institutions regarding royalties from natural resources and their allocation to subnational governments. According to Viale and Cruzado (2012), Bolivia, Brazil and Peru allocate a share of income of extractive industries directly to subnational governments, as Colombia does. Ecuador and Venezuela also use payments from the exploitation of natural resources to compensate producing regions. In all the countries mentioned, there are restrictions on how the income from the extraction of natural resources may be used. Otto et al. (2006) note that it is standard practice in most African and Asia-Pacific countries to levy royalties on mineral extraction.

[11] The minimum coverage goals for health, child mortality, education, and water are respectively: 100% of health insured population, less than 1% of child mortality before reaching the second year of life, 90% of school enrollment, and 70% of the population with access to clean water.

3.4 Data

To pursue our analysis of spatial interactions in municipal spending, we require four types of data: Public spending data at the local level, varying over time and across municipalities; information on municipalities' oil extraction; time-series data on world market oil prices; and cross-sectional geo-information on the municipalities' locations to construct spatial weighting matrices.

We retrieve local public spending data from the Center for Studies on Economic Development (CEDE) of the University de los Andes, which in turn collected the information from government agencies. The data contain a wide range of government-related variables from the full universe of Colombian municipalities over the years 1993 to 2013. This dataset has previously been used to analyze the effects of fiscal decentralization (Soto et al., 2012), income shocks (Dube and Vargas, 2013) and political stability (Acemoglu et al., 2013), among other studies. As full coverage is not given for all years and to avoid large differences in the institutional setting, we focus on the period between 2000 and 2010. This results in a strongly balanced panel of 1093 municipalities over eleven years. The data allow us to differentiate between eleven major spending categories at the municipality level. We express all variables in real per capita Colombian Pesos (COP) with the base year 2008 if not stated otherwise.

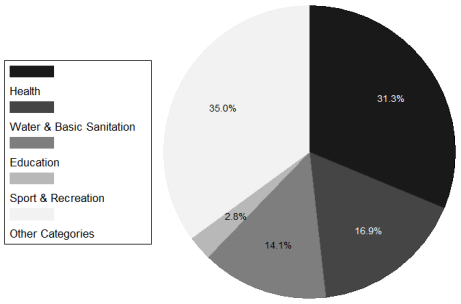
Figure 3.1a shows the largest expenditure categories in terms of their average shares in total local public spending. About 31% of a municipalities' expenditures go into the area of *health*. Spending in *water and basic sanitation* as well as in *education* account for another 17% and 14%, respectively. The remaining funds are spent on *sport and recreation*, *housing*, *attention to vulnerable groups*, *municipal facilities*, *agriculture*, *disaster prevention*, *community development*, *justice and security* as well as further smaller categories. Figure 3.1b shows that there was an increase in local real expenditures since the year 2000, mirroring the Colombian decentralization process. Table B.1 in Appendix B.1 describes the spending categories in more detail and provides some examples.

The data are very complete and have an exceptionally high quality for a developing country context. We did not detect systematic trends of missing values and had to impute less than 3% of the observations by linear intrapolation (see Appendix B.2). From CEDE, we also retrieve the total transfers to municipalities from the central government and other sources and the municipalities' total and rural population.

We retrieve information on oil extraction by private companies within a municipality from the Ministry of Finance. To get an exogenous indicator of oil extraction, we create a binary variable equaling one if oil was extracted on a municipality's soil at any time during the years 1990 to 1999, the decade preceding our analysis.

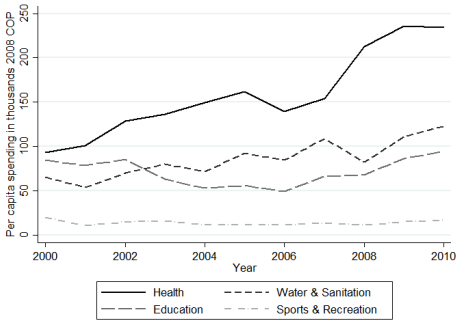
To obtain a measure of the value of the oil extracted, we use the oil basket price

Figure 3.1: Local public expenditures of Colombian municipalities



Note: Relative shares of spending types per capita, averaged over 2000-2010.

a. Relative shares of expenditure categories



Note: Annual averages across categories, 2000-2010.

b. Expenditure levels over time

in USD provided by OPEC. According to Colombian law,[12] the exchange rate used to calculate the value of the royalties is the average of the daily official exchange rate between the USD and the COP calculated by the Financial Superintendence of Colombia. Thus, we convert oil prices into COP applying the official exchange rate. Finally, we collect the cross-sectional geographical information on municipalities from the Colombian Geographic Institute Agustin Codazzi (IGAC).

Table 3.1 summarizes descriptive statistics of the major variables from the final sample. Figure 3.2 depicts the spatial distribution of local total public expenditures in Colombian municipalities and suggests spatial clustering. In our econometric analysis we investigate whether this is due to causal expenditure interactions.

3.5 Empirical Strategy

3.5.1 Model of Spatial Expenditure Interaction

To estimate the effect of neighboring municipalities' expenditures on the spending of the focal municipality, we consider the following model:

$$Y_{i,d,t} = \delta WY + \eta\ oil_i \times p_t^{oil} + WX\theta + X_{i,t}\beta + \alpha_i + \tau_{d,t} + \varepsilon_{i,d,t} \tag{3.2}$$

where $Y_{i,d,t}$ is the natural log of local public spending per capita (total spending or one of the spending categories) in municipality i of department d in year t. WY denotes the spending in neighboring municipalities (total or in the respective category), where W is a spatial weighting matrix. The coefficient of interest is the spatial autocorrelation coefficient δ, which measures potential spatial interaction effects. The dummy oil_i equals 1 if oil was exploited within a municipality between

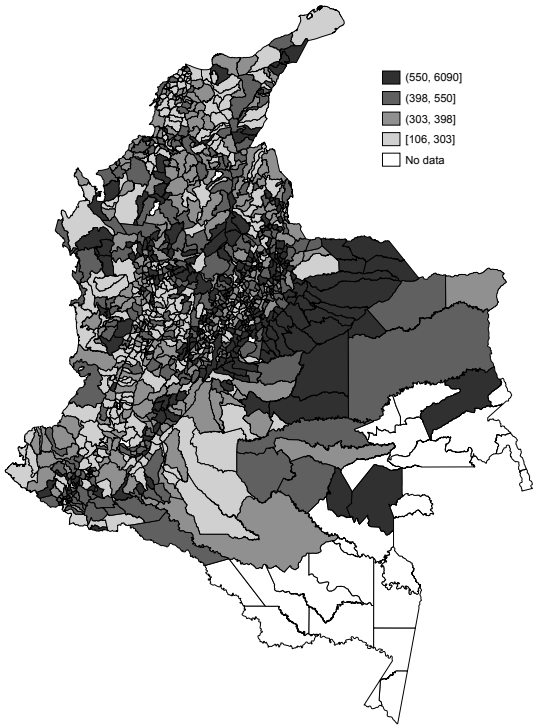
[12] Law 141 of 1994, law 756 of 2002.

Table 3.1: Descriptive Statistics

	Mean	Std. dev.
Total Expenditures	507.92	441.15
Health	158.87	89.92
Water and Basic Sanitation	85.84	137.99
Education	71.46	101.10
Housing	14.05	32.87
Sport and Recreation	13.99	23.53
Agriculture	12.77	22.40
Municipal Facilities	12.32	29.90
Attention to Vulnerable Groups	10.60	17.08
Justice and Security	5.59	12.45
Disaster Prevention	3.99	14.21
Community Development	1.72	7.94
Federal Transfers	390.42	268.46
Oil production status (1990-99)	0.06	0.24
Population in thousand inhabitants	39.08	235.98
Rural population share	582.27	236.14

Notes: The table provides averages over the entire sample period 2000-2010. Expenditures and transfers are real per capita values in thousand 2008 COP. $N = 1093$ municipalities.

Figure 3.2: Total expenditures per capita in thousand COP by quartiles in 2005



Notes.– In all figures, "No data" refers to entities not used in our analysis, see Appendix B.2.

1990 and 1999. p_t^{oil} is the natural log of the world market price of oil converted to real COP. The interaction of the previous two variables captures differential effects of oil price changes on oil endowed and non-endowed municipalities primarily due to the receipt of royalties. The vector $X_{i,t}$ collects additional time-variant covariates: Total spending of a municipality on all categories other than the focal category Y, population of the municipality and its square, the local share of the rural population, and transfers from higher levels of government (including indirect royalties). We also include the spatial lags WX of all X variables (Spatial Durbin Model). This accounts for the possibility that changes in neighboring municipalities such as population growth directly influence a focal municipality's spending. Municipality fixed-effects α_i capture time-invariant unobserved factors such as the distance to the capital, geographical size and climatic conditions. Department-year fixed effects $\tau_{d,t}$ control time variation that affects all municipalities within a department in the same way such as regional economic shocks and changes in department-level regulations and funding.

3.5.2 Oil Price Shocks as Exogenous Variation in Local Spending

In Equation (3.2), WY is endogenous due to the simultaneous influence of neighboring municipalities on one another. Furthermore, unobserved factors that change over time with variation within departments are contained both in WY and the error term $\varepsilon_{i,d,t}$. To deal with this endogeneity, our empirical strategy employs an instrumental variable approach that isolates exogenous variation in municipal spending.

Similar to Acemoglu et al. (2013), we exploit oil price shocks that affect the finances of some but not all municipalities. We do so by combining information on oil endowments of municipalities with variation in oil prices on the world market over time to extract quasi-experimental variation in spending changes of neighboring municipalities.

In the 2SLS estimation, the first stage is given by

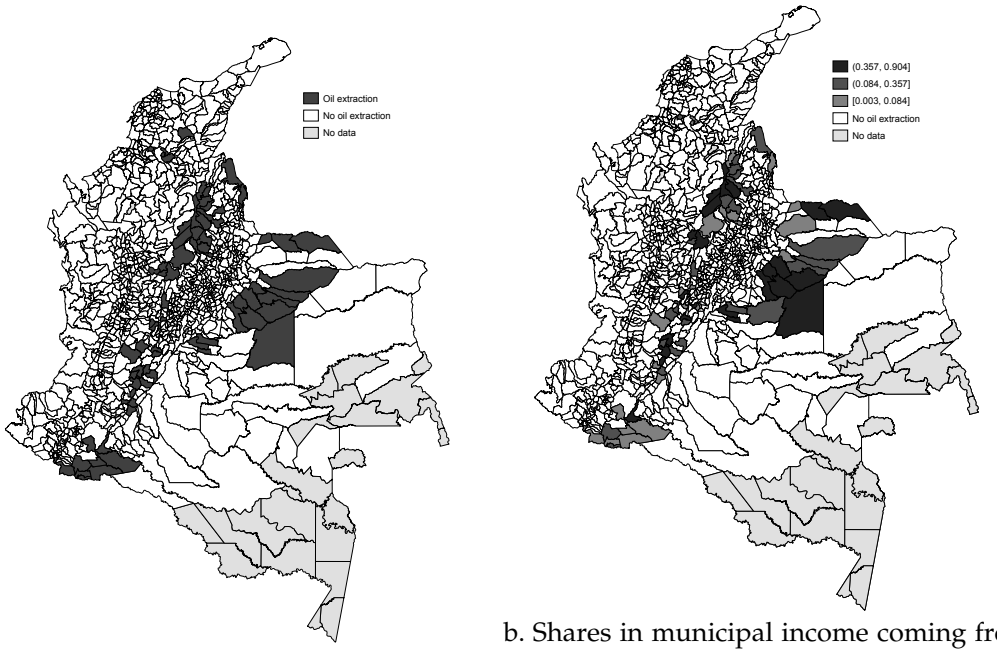
$$WY = \gamma\ oil_i \times p_t^{oil} + Woil \times p_t^{oil}\lambda + WX\kappa + X_{i,t}\rho + \alpha_i^f + \tau_{d,t}^f + \vartheta_{i,d,t} \tag{3.3}$$

where neighboring oil endowment (in the 1990s, before the period of analysis) interacted with current international oil prices in real COP ($Woil \times p_t^{oil}$) is used as the instrument for neighboring spending.

The validity of the instrument relies on two requirements. First, the combination of oil endowment and oil prices must be correlated with local spending (instrument relevance). Oil prices are linked to local spending as Colombian municipalities receive royalties from oil extracted on their soil, relaxing their budget constraint.

Federal law mandates royalty payments depend on extracted quantities as well as current world market prices (see Section 3.3). Figure 3.3 suggests that oil production in the 1990s (Panel a) corresponds well with oil royalty income in the middle of our period of analysis (Panel b). Therefore we expect the instrument to be relevant for total local public expenditures and its components. To obtain a strong instrument, the movements of the global oil price must provide sufficient variation to substantially influence spending of oil-endowed municipalities. Depicting the evolution over time, Figure 3.4 illustrates that oil prices were highly volatile over the sample period and, thus, introduce plenty of variation.

Figure 3.3: Geography of oil production and municipal oil revenues



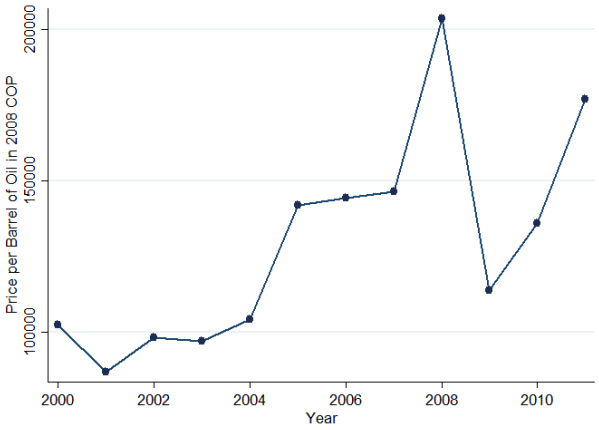
a. Municipalities producing oil (1990-1999)

b. Shares in municipal income coming from oil royalties by terciles conditional on positive oil royalties (2005)

Concerning single expenditure categories, municipalities are supposed to spend 75% of the royalties for the earmarked categories health, education and water as long as coverage goals have not been achieved, as explained in Section 3.3. Therefore, we expect the instrument to be strong for the earmarked categories. As mentioned before, royalties can potentially also be used for expenditures in non-earmarked categories, so we test the strength of the instruments for these categories as well. Statistical tests of the strength of the instrument are satisfactory for total expenditures and the earmarked categories but not for most non-earmarked categories; we discuss this in more detail in Section 3.6.1.

The second requirement for a valid instrument is that conditional on the covariates included, the IV must not correlate with the error term in the second stage (exogeneity assumption). As we control for municipality fixed effects, time-

Figure 3.4: Oil price in 2008 real COP (OPEC basket)



invariant differences between oil-endowed municipalities and other municipalities are accounted for. The spatial distribution of oil reserves is not under the control of local governments. Potentially endogenous efforts in oil discovery do not affect our identification because we use oil extraction indicators from 1990 to 1999, the decade preceding our period of analysis. Moreover, municipalities cannot manipulate the extracted quantities as private companies negotiate drilling contracts with the central government. Federal law regulates the royalties paid to the municipalities. Regarding the oil price on the world market, municipalities in Colombia are price takers. Although crude oil is the country's most important export good, Colombia does not rank among the major exporting nations and is not part of OPEC.

The spatial lag of a municipality's total expenditures (excluding the focal category Y) is a potentially important control variable, because oil royalties received by neighboring municipalities are likely to influence their spending not only in the focal category but also in other categories. If these other spatially lagged expenditures were omitted from the regression, our instrument could potentially be correlated with the error term in the second stage equation. In Section 3.6.3, we explore the sensitivity of our results when we treat total expenditures and its spatial lag as endogenous and when we include spending in all categories and their spatial lags separately in the regression.

In Table 3.2, we assess the structural similarity of oil-endowed and non-endowed municipalities using an additional source of data, the Colombian census conducted in 2005. The comparison shows that the two groups of municipalities are observationally equivalent concerning demographic and socio-economic characteristics. The good balance indicates that both municipality types are very similar except for their oil endowments, which supports our identification strategy. Figure 3.3a depicts the two groups of municipalities within a map of Colombia. This map reveals that oil extracting municipalities are somewhat clustered but sufficiently scattered over the country to obtain broad coverage of our instrument.

Table 3.2: Treatment - Control Balance in Observables

Variable	Mean		t-test
	Endowed	Non-endowed	*p*-value
Poverty rate	0.69	0.69	0.92
Informal employment share	0.95	0.95	0.51
Economic dependence share	0.54	0.54	0.81
Illiteracy share	0.28	0.29	0.46
Child labor share	0.06	0.06	0.59
Children w/o access to education, share	0.14	0.13	0.54
Children w/o access to care services share	0.21	0.20	0.56
Household share w/o health insurance	0.28	0.30	0.35
Household share w/o access to health	0.09	0.09	0.42
Household share w/o access to clean water	0.36	0.37	0.66
Household share w/o a sewage system	0.31	0.34	0.21
Household share w/o floors	0.19	0.21	0.27
Household share w/o walls	0.07	0.07	0.63

Notes.– All rates and shares refer to the households within a municipality. A municipality is defined as endowed if oil was extracted on its soil at any time between 1990 and 1999. Means are unweighted averages across municipality groups. The table shows *p*-values for *t*-tests of equal means between the two groups. Variables based on the Colombian 2005 census (cross-section).

Our estimation approach differs from the traditional spatial IV estimator. The latter uses all spatially lagged covariates WX to instrument the neighboring endogenous variable WY, in our case spending (see Anselin, 2008, for an overview). However, whether all WX can be excluded from the second stage equation is questionable (Gibbons and Overman, 2012; Revelli, 2015). For example, population changes in neighboring municipalities may well have a direct effect on spending decisions of the focal municipality or be correlated with spatially clustered time-varying unobserved factors, which would invalidate the traditional instruments. Therefore, we safely control for all WX in the second stage equation. The only excluded instrument is the interaction term of oil endowment of neighboring municipalities before the observation period with the international oil price. As we control for the interaction of the oil endowment dummy of the focal municipality with the oil price as well as municipality and department-time fixed effects in the second stage, the argument of exogeneity of this selected instrument is very strong. In Section 3.6.2, we compare the results from our preferred IV estimator with those from the arguably inconsistent traditional IV estimator.

To construct the spatial weighting matrix, we use the 5-nearest neighbor (NN) criterion as our main approach because the average municipality has five neighbors. As robustness checks, we compare the results to estimations using 4-NN and 6-NN matrices and an economic proximity matrix based on municipalities' average per capita income. To safeguard against potentially remaining serial correlation in the error term even after eliminating the unobserved fixed effects, we report standard errors clustered at the municipal level throughout the paper.

We contrast our IV results with the traditional spatial econometric estimators that use the Quasi Maximum-Likelihood method to estimate fiscal spillovers (Anselin, 2010). We follow the model selection procedure suggested by Elhorst (2010). Starting from OLS and going through the Spatial Lag (SAR), Spatial Error (SEM) and Spatial Durbin Model (SDM), we test hypotheses on whether and what kind of spatial terms should be included. The tests indicate that the SDM is the preferred model and thus confirm our initial model choice.[13] Thus, we estimate Equation (3.2) by QML, but without the oil-related variables.[14] For the QML-estimation to be consistent, knowledge of the true data generating process including the spatial weights must be assumed, as criticized by Gibbons and Overman (2012). Therefore, we prefer our IV estimator and present results from the potentially inconsistent QML estimator for comparison only.

3.6 Empirical Results

3.6.1 Results Based on the Quasi-Experimental Instrument

Table 3.3 reports the main (second stage) results from estimations of Equation (3.2) using our preferred IV estimator. In this table we present estimates for total local public expenditures, the three categories earmarked for spending royalty income, all non-earmarked categories combined, as well as the specific non-earmarked category sport and recreation. The first row contains the estimated parameter of interest δ, which captures spatial public spending interaction. Our results indicate no significant spatial interactions in total expenditures, the earmarked categories, and the non-earmarked categories combined. The point estimate of spatial interactions for total local public spending is particularly close to zero (-0.081). The exception is the category of local public spending for sport and recreation, where local governments respond significantly to spending decisions of neighboring municipalities; we discuss this result in more detail below.

The control variables confirm expectations. Higher total expenditure as a measure of a municipality's budget (excluding the focal spending category to avoid endogeneity) increases spending in the focal category as well. Similarly, higher transfers from higher levels of government increase total spending and spending in all categories. When the population in a municipality grows, total public expendi-

[13] Detailed results are available from the authors on request.

[14] Moreover, for computational reasons, the year fixed effects are not department-specific in the QML estimations. This does not drive our results, though, because the IV estimates remain similar if we use country-level instead of department-level year fixed effects, see Section 3.6.3. In order to make our IV and QML estimations comparable, we report estimations based on the same sample in our tables. Therefore, we do not apply the bias corrected QML estimator suggested by Lee and Yu (2010) because this would imply losing one year of observations. However, we obtain similar results when implementing the bias correction. The small impact of the bias correction can be explained by our relatively large sample size.

Table 3.3: Main Estimation Results of Expenditure Interactions

	Total	Health	Education	Water	Non-earmarked	Sport & rec.
W_y	-0.081	-0.140	0.282	0.358	0.492	1.257**
	(0.499)	(0.540)	(0.238)	(0.413)	(0.663)	(0.576)
Oil extraction x oil price	0.302***	0.405***	0.921***	0.505***	-0.277**	-0.185
	(0.080)	(0.117)	(0.135)	(0.157)	(0.115)	(0.129)
Total excl. y		0.260***	0.418***	0.587***	0.108***	0.555***
		(0.035)	(0.034)	(0.034)	(0.031)	(0.030)
W_total excl. y		0.087	-0.059	-0.147	-0.047	-0.702**
		(0.129)	(0.114)	(0.261)	(0.076)	(0.303)
Population	6.175***	4.625	16.350***	-7.207**	0.808	0.003
	(2.359)	(3.373)	(4.668)	(3.001)	(3.168)	(3.823)
Population squared	-2.996***	-2.120	-7.159***	3.191**	-0.768	-0.287
	(1.088)	(1.564)	(2.161)	(1.402)	(1.482)	(1.781)
Share of rural population	-0.596***	-0.377**	-1.147***	0.027	-0.182	0.139
	(0.144)	(0.166)	(0.244)	(0.164)	(0.176)	(0.165)
Transfers	0.029***	0.012***	0.020***	0.017***	0.027***	0.012***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)	(0.003)
W_population	0.444	-2.140	-10.329	1.750	0.166	-4.647
	(5.820)	(6.037)	(8.862)	(6.878)	(6.885)	(8.744)
W_population squared	0.136	1.119	5.378	-0.628	0.294	2.520
	(2.678)	(2.806)	(4.028)	(3.189)	(3.336)	(4.173)
W_share of rural population	-0.508	-0.082	-0.637	0.177	0.003	0.086
	(0.430)	(0.428)	(0.516)	(0.408)	(0.357)	(0.334)
W_transfers	0.008	-0.006	-0.009	-0.016	-0.004	-0.014*
	(0.018)	(0.010)	(0.009)	(0.011)	(0.026)	(0.009)
Observations	12,023	12,023	12,023	12,023	12,023	12,023
Number of municipalitites	1,093	1,093	1,093	1,093	1,093	1,093
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes
Department - year FE	Yes	Yes	Yes	Yes	Yes	Yes
AP F-statistic excl. inst.	16.26	22.25	83.01	24.09	9.035	13.99

Notes: Second stage IV estimation results of spatial expenditure interactions. The columns show results for different local public expenditure variables *Y*. Excluded IV for *WY*: Endowment of neighboring municipalities with oil in the 1990s interacted with the current world market price for oil. 5-NN spatial weighting matrix. Expenditures and transfers are real per capita values in 2008 COP. All continuous variables in logs. The Angrist-Pischke (AP) first-stage F-statistic of the excluded instrument is also reported. Standard errors given in parentheses are clustered at the municipality level. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

tures increase, but at decreasing marginal rates, which is likely due to economies of scale. A larger share of rural population decreases local public spending.

The F-statistic of excluded instruments is sufficiently large in all columns of this table except for the non-earmarked composite. Table 3.4 presents the corresponding first stage results in more detail. As expected, the interaction of oil extraction of neighboring municipalities with the oil price is positive and highly significant when total spending or the earmarked spending categories of the neighboring municipalities are the dependent variables in the first stage. This indicates that municipalities spend more on the earmarked categories and also spend more in total when they receive more royalties from the extraction of oil. Thus, they largely seem to follow the federal laws regulating the spending of royalty income. In contrast, neighboring municipalities spend less on the non-earmarked categories when they receive more oil royalties. A potential explanation is that the increased expenditures in earmarked categories triggered by higher royalty income directs attention of local politicians and thus spending away from non-earmarked categories.[15]

Table 3.4: Main Specification: First Stage

	W_total	W_health	W_education	W_water	W_non-earmarked	W_sport & rec.
Oil extraction x oil price	-0.020	0.035	-0.034	0.026	-0.045	-0.095**
	(0.031)	(0.042)	(0.058)	(0.061)	(0.049)	(0.041)
W_Oil extraction x oil price	0.281***	0.361***	0.980***	0.582***	-0.291***	-0.322***
	(0.070)	(0.076)	(0.108)	(0.119)	(0.097)	(0.086)
Total excl. y		-0.004	0.011	-0.000	0.010**	-0.014*
		(0.007)	(0.008)	(0.010)	(0.005)	(0.008)
W_total excl. y		0.234***	0.462***	0.622***	0.109***	0.522***
		(0.019)	(0.021)	(0.023)	(0.015)	(0.019)
Population	-0.121	-0.221	-0.063	1.305	-0.918	0.632
	(1.279)	(1.507)	(1.650)	(1.513)	(1.908)	(1.467)
Population squared	0.127	0.155	0.226	-0.596	0.458	-0.270
	(0.600)	(0.702)	(0.770)	(0.710)	(0.901)	(0.687)
Share of rural population	-0.032	-0.066	-0.228***	-0.094	0.121	0.042
	(0.058)	(0.063)	(0.083)	(0.081)	(0.075)	(0.074)
Transfers	0.001	-0.001	-0.001	-0.000	0.003**	-0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
W_population	8.399***	3.912	11.793***	-4.401	6.386*	7.630***
	(2.279)	(3.008)	(3.567)	(3.094)	(3.423)	(2.935)
W_population squared	-3.867***	-1.835	-4.501***	2.103	-3.287**	-3.797***
	(1.062)	(1.404)	(1.654)	(1.443)	(1.599)	(1.363)
W_share of rural population	-0.691***	-0.586***	-1.685***	-0.458***	0.070	0.178
	(0.121)	(0.135)	(0.181)	(0.147)	(0.149)	(0.140)
W_transfers	0.036***	0.012***	0.021***	0.020***	0.039***	0.008***
	(0.002)	(0.003)	(0.003)	(0.004)	(0.003)	(0.003)
Observations	12,023	12,023	12,023	12,023	12,023	12,023
R-squared	0.069	0.042	0.212	0.126	0.049	0.113
Number of municipalities	1,093	1,093	1,093	1,093	1,093	1,093
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes
Department - year FE	Yes	Yes	Yes	Yes	Yes	Yes
AP F-value excl. inst.	16.26	22.25	83.01	24.09	9.04	13.99

Notes: First stage IV results; the second stage is reported in Table 3.3. Expenditures and transfers are real per capita values in 2008 COP. All continuous variables in logs. The Angrist-Pischke (AP) first-stage F-statistic of the excluded instrument is also reported. Standard errors given in parentheses are clustered on the municipality level. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

[15]For the strength of the instrument, it is only important that it is strongly correlated with spending of neighboring municipalities; the sign of the relationship is irrelevant.

Table B.2 in Appendix B.1 shows the second-stage results for all single non-earmarked categories, where each row represents a separate estimation for the indicated category. We only present the most relevant coefficients and statistics in the columns. For most of these non-earmarked categories, the F-statistic indicates that the instrument is weak, except for the categories sport & recreation and municipal equipment. Thus, our identification strategy allows us to identify the presence of expenditure interactions for total expenditures and the earmarked categories as well as for the two non-earmarked categories mentioned. For municipal equipment, we do not find significant spatial spending interactions.[16]

For the category of sport and recreation, the point estimate of the significant coefficient of spatial interaction is larger than one, indicating an explosive process. When the five nearest neighboring municipalities increase their spending for sport and recreation on average by 1%, this causes the focal municipality to increase its spending in the same category by 1.25%. Given that municipalities spend a comparably small share of their total budget for sport and recreation (see Figure 3.1a), a temporarily explosive pattern during our period of analysis is not implausible.[17] However, the finding of an instable spatial process should be regarded with caution for two reasons. First, a t-test fails to reject the hypothesis that the coefficient is smaller than one (p-value: 0.33), so we do not rule out that the process is stable. Second, when using certain alternative spatial weighting matrices in robustness checks reported in Section 3.6.3, the point estimate of the coefficient drops below one. Figure B.1 in Appendix B.1 shows that municipal spending on sport and recreation is clearly clustered both at the beginning and end of our period of analysis, which is consistent with the presence of fiscal interactions, but it is unclear weather these interactions are explosive during this time period.

As there is no spatial interaction in total local spending, the significant interaction in the spending category sport and recreation reflects a change in the composition of the local budget. The finding of fiscal interaction in this area reflects the importance of sports and recreational activities all over Colombia.[18] At least three factors may explain the presence of expenditure interactions in this spending category. Firstly, because this category includes items such as playgrounds or sports fields and instructors, these expenditures are very visible to the local voters and could therefore be used by incumbent local politicians to secure votes in future municipal elections, as argued by Drazen and Eslava (2010). Because of its visibility and voters'

[16] There appears to be weak evidence of significant fiscal interactions also in the category of spending for justice and security, but this result must be interpreted with caution because of the weak instrument for this category.

[17] The literature on spatial competition in local public welfare spending occasionally reports spatial interaction coefficients larger than one as well (Figlio et al., 1999; Saavedra, 2000).

[18] Case studies indicate that Colombian municipalities devote a high proportion of resource royalties to areas like entertainment and sports complexes (Gaviria et al., 2002). We additionally run separate estimations for different regions of Colombia and find that the statistically and economically strongest spatial interactions in spending for sport and recreation occur in the east of the country.

awareness, spending in this category may be used primarily in yardstick competition. Secondly, positive interactions could also arise because of complementarities in expenditures in this category between neighboring municipalities. For example, neighboring municipalities might cooperate in certain sports events or programs or jointly build and use sports stadiums. A third reason for the interaction could be competition between municipalities to attract a mobile tax base. For example, bigger and better recreational facilities might attract businesses to the municipality that value employee wellbeing. In this case, municipalities have an incentive to respond to expenditures of their neighbors in this category. Each of these explanations is consistent with the positive estimated interaction effect.

3.6.2 Classical Spatial Econometric Approaches

For comparison with our preferred IV estimates, Table 3.5 presents the estimation results using the traditional QML estimator with municipality and year fixed effects. Based on this estimator that does not exploit quasi-experimental variation, the results suggest highly significant spatial autocorrelation between neighboring municipalities in total local public expenditures as well as all the earmarked categories and the combined non-earmarked categories (as well as sport and recreation).[19]

[19] This also holds when we use the SAR or SEM models or different weighting matrices.

Table 3.5: QML Results of Expenditure Interactions

	Total	Health	Education	Water	Non-earmarked	Sport & rec.
W_y	0.182***	0.116***	0.148***	0.126***	0.147***	0.0685***
	(0.0147)	(0.0157)	(0.0158)	(0.0148)	(0.0148)	(0.0148)
Total excl. y		0.262***	0.433***	0.587***	0.116***	0.534***
		(0.0350)	(0.0343)	(0.0367)	(0.0304)	(0.0263)
W_total excl. y		-0.000665	0.0537	-0.00902	0.0198	-0.0635
		(0.0357)	(0.0390)	(0.0455)	(0.0199)	(0.0389)
Population	6.910***	4.293	19.30***	-5.039	1.138	1.479
	(2.453)	(3.619)	(4.936)	(3.502)	(3.164)	(3.237)
Population squared	-3.360***	-1.994	-8.497***	2.200	-0.959	-1.001
	(1.129)	(1.672)	(2.281)	(1.642)	(1.470)	(1.495)
Share of rural population	-0.501***	-0.313*	-0.986***	0.0308	-0.0938	0.261*
	(0.147)	(0.170)	(0.246)	(0.152)	(0.159)	(0.150)
Transfers	0.0283***	0.0135***	0.0177***	0.0165***	0.0271***	0.0101***
	(0.00259)	(0.00330)	(0.00330)	(0.00361)	(0.00359)	(0.00299)
W_population	-4.206	-9.690*	-8.972	7.029	1.196	4.388
	(4.083)	(5.759)	(7.909)	(6.884)	(5.558)	(6.673)
W_population squared	2.284	4.705*	4.887	-3.101	-0.462	-2.115
	(1.873)	(2.664)	(3.668)	(3.191)	(2.575)	(3.099)
W_share of rural population	-0.00124	0.138	-0.101	0.255	0.116	0.423
	(0.240)	(0.267)	(0.310)	(0.312)	(0.308)	(0.280)
W_transfers	-0.00285	-1.20e-05	-0.0156**	-0.0129**	0.00313	-0.00649
	(0.00441)	(0.00718)	(0.00634)	(0.00650)	(0.00672)	(0.00545)
Observations	12,023	12,023	12,023	12,023	12,023	12,023
Number of municipalitites	1,093	1,093	1,093	1,093	1,093	1,093
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: QML estimation results of spatial expenditure interactions. The columns show results for different local public expenditure variables Y. Expenditures and transfers are real per capita values in 2008 COP. All continuous variables in logs. Standard errors given in parentheses are clustered on the municipality level. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

From these estimates alone, it would be tempting to conclude that significant spatial interaction is present in all these categories of local public expenditures. However, the results from using exogenous variation in the previous section show that this spatial autocorrelation does not indicate causal effects. This methodological comparison highlights the importance of relying on quasi-experimental variation in the causal analysis of fiscal interactions.

As another comparison, Table B.3 in Appendix B.1 provides results from the traditional spatial IV estimator that uses spatial lags of all control variables as excluded instruments. As argued in Section 3.5.2, we expect this estimator to be inconsistent, like the QML estimator. The results suggest significant spatial interaction in local spending on education. However, our preferred IV estimator that uses quasi-experimental variation suggests that this correlation is spurious.[20]

3.6.3 Robustness Checks

We assess the sensitivity of the results from our preferred IV estimator by employing alternative weighting matrices and specifications. Table 3.6 summarizes the first set of robustness checks for total local public expenditures (first two columns) and local spending for sport and recreation (two rightmost columns). In the first three table rows, we vary the number of neighbors included for constructing the k-nearest neighbor weighting matrix. In row four, we use an inverse distance weighting matrix where distance is defined in an economic way by the difference in income between municipalities.[21] The results show that fiscal interactions in total spending are always insignificant.[22]

The point estimates also indicate strong spatial interactions in sport and recreation in all specifications. Only when using the 6-NN matrix, the coefficient loses statistical significance, suggesting that interactions cannot be detected when neighbors are too far away. When using the 6-NN matrix or the inverse income distance matrix, the point estimate of spatial interaction drops below one, which indicates a stable process, in contrast to our baseline result of an explosive process. Thus, while we find robust evidence of strong spatial interactions in local public spending for sport and recreation, we cannot decide with high confidence whether this process is stable or explosive.

In some spending categories, a number of municipalities report zero spending in some years, for example in the sport and recreation category. To avoid dropping these municipalities, in the baseline specification we add one to all spending

[20] Furthermore, when using the traditional spatial IV estimator, the point estimate of the spatial interaction in total local public spending increases, but remains insignificant, and the spatial interaction in sport and recreation loses significance.

[21] Geographical distance is not suitable in the Colombian context because municipalities vary extremely in their area size, see Figure 3.2.

[22] This also holds when we run separate estimations for different regions within Colombia using our baseline 5-NN weighting matrix.

Table 3.6: Robustness Checks: Different Matrices and Specifications

	Total expenditures		Sport & recreation	
Specification	W_y	AP F-statistic	W_y	AP F-statistic
4-NN weighting matrix	-0.010	13.09	1.188**	17.49
	(0.499)		(0.539)	
5-NN weighting matrix	-0.081	16.26	1.257**	13.99
	(0.499)		(0.576)	
6-NN weighting matrix	-0.132	22.87	0.859	23.30
	(0.496)		(0.533)	
Inv. income distance w. matrix	0.526	11.17	0.729*	7.976
	(0.403)		(0.408)	
Inverse hyperbolic sine	-0.121	16.94	1.284**	13.14
	(0.501)		(0.602)	
Country-level year fixed effects	-0.294	18.68	1.188***	28.40
	(0.548)		(0.375)	
Controlling for conflict	-0.099	15.82	1.263**	13.32
	(0.510)		(0.594)	

Notes: Each row represents a different model specification and shows the spatial autocorrelation coefficients for total local public spending and sport & recreation as dependent variables (separately estimated) with the corresponding Angrist-Pischke (AP) first-stage F-statistics of the excluded instrument to the right. Excluded IV for *WY*: Endowment of neighboring municipalities with oil in the 1990s interacted with the current world market price for oil. Expenditures and transfers are real per capita values in 2008 COP. All continuous variables in logs. Standard errors given in parentheses are clustered on the municipality level. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

variables before taking the logarithm. In the robustness check in row five of Table 3.6, we use the inverse hyperbolic sine transformation instead (again using our preferred 5-NN weighting matrix). This function is similar to the logarithm, but defined for values of zero as well. The results are very similar to the baseline results.

To present a specification fully comparable with our QML estimation, we also run a version of our main IV model where year fixed effects replace department-year fixed effects. The estimates show that this alternation leaves the main results unchanged.

Next, we assess whether the Colombian civil war that started in the 1960s influences fiscal interactions. The violence was particularly severe during the late 1990s and early 2000s. Civil conflicts in a certain region could induce spatially correlated public spending patterns. Therefore, we include civil war related casualties in the municipality and its spatial lag as additional covariates in our model. Following Dube and Vargas (2013), we retrieve the casualties variable from the Conflict Analysis Resource Center (CERAC) which provides the most comprehensive and independent source of civil war related data. However, the coefficients of these additional controls turn out to be insignificant, and the inclusion of the variables does not affect the estimates of the fiscal interaction coefficients. The inclusion of the variables does not alter the findings in the QML specifications either (results

not tabulated). Thus, our results are robust to the inclusion of civil war related variables.[23]

Another concern regarding our model could be potential endogeneity of the total expenditure covariate and its spatial lag. Although we exclude the category of interest Y from the total expenditure controls to avoid a mechanical correlation with the dependent variable, one may be concerned about simultaneity of choices. Therefore, for these two potentially endogenous covariates, we consider using their one-year time lags as excluded instruments, assuming that the error terms in Equation (3.2) are serially uncorrelated. On this basis we conduct a Hausman test of endogeneity of the two covariates.[24] The test results indicate that the null hypothesis of exogeneity of total expenditure and its spatial lag cannot be rejected. Therefore, our more efficient baseline estimator that treats these controls as exogenous is preferred.

We also estimate more flexible specifications where we include each of the ten other public spending categories and their spatial lags separately in the model as control variables when a specific spending category is the dependent variable. Table B.4 in Appendix B.1 reports the results; we do not report the coefficients of the spatial lags of the spending categories for brevity. The findings confirm that there are no significant spatial interactions for the earmarked categories, and they also replicate the size and significance of the interaction in spending for sport and recreation.[25] In summary, we conclude that the main results are robust to specification choices as long as identification relies on our quasi-experimental instrument.

3.7 Conclusion

This paper investigates spatial interactions in local public expenditures in a developing country applying a quasi-experimental identification strategy. We use panel data on the universe of the more than 1000 municipalities in Colombia over a period of eleven years to estimate spatial interaction effects in total local expenditures and the eleven most important spending categories. For identification, we rely on exogenous variation in the exposure of individual municipalities to shocks in oil prices on the world market due to their local endowment of oil resources.

For total local public expenditures and most spending categories, the estimates

[23] As we cannot rule out potential endogeneity of civil conflict, we do not employ this covariate in our main specification but only in this robustness check.

[24] We run this test for the spending category of sport and recreation, where we found the significant spatial interaction effect.

[25] However, this specification does not seem to be suitable for the health category, where the standard error and the point estimate increase a lot. When we treat all expenditure categories and their spatial lags as endogenous and instrument them with their on-year time lags, the point estimate of the spatial interaction in health spending decreases to -0.08, so the large point estimate seems to be biased.

of spatial interactions are small and not significantly different from zero. The notable exception is public expenditures for sport and recreation, where we detect significant and large causal spatial interactions. In contrast, when we use spatial econometric estimators that do not employ quasi-experimental variation, we find strong and significant spatial autocorrelation in almost all categories of public expenditures. Our comparison of methods shows that this spatial autocorrelation cannot be interpreted causally. Therefore, our results highlight the importance of using additional exogenous sources of variation for causal inference on fiscal interactions. This is in line with the results of Isen (2014) concerning spending competition in Ohio and of Lyytikäinen (2012) regarding tax competition in Finland although we do find evidence for significant causal expenditure interactions in the spending category of sport and recreation.

Our findings have important policy implications. The results demonstrate that fiscal decentralization in developing countries does not necessarily lead to a general race to the bottom concerning local public expenditures due to strategic interactions of local governments. Our findings lend support to decentralization reform in developing countries as they alleviate concerns about inefficiencies implied by fiscal externalities. On the other hand, one also cannot expect that innovative expenditure policies rolled out in a municipality, perhaps in the context of decentralized foreign aid, will spread out substantially to neighboring municipalities through mimicking of local expenditure policy.

A caveat is in order when applying our findings to other countries. Although Colombian municipalities have considerable leeway in their expenditure decisions, spending rules for royalties and transfers may restrain fiscal interactions in Colombia. Nonetheless, many developing countries have similar institutions in this respect. Countries such as Bolivia, Brazil and Peru directly allocate a share of income from extractive industries to subnational governments, as Colombia does. Most countries that levy royalties on the extraction of natural resources also impose restrictions on how this income can be used. Thus, the case of Colombia is illustrative in a larger context.

More research is needed to investigate whether our findings hold in other countries with and without income from natural resources, the allocation of royalties to subnational governments and earmarking. Future research should also explore different quasi-experimental setups and spatial discontinuity designs, ideally within the same institutional environment, and additional fiscal variables. Another avenue for further research is to address the significant causal spatial interactions in local public expenditures for sport and recreation we found and to examine the mechanisms leading to this novel empirical result.

4 Privatization Decisions and Economic Efficiency: Evidence from Mass Privatization

4.1 Introduction

Privatization is a widely-used policy tool for governments worldwide. Global privatization revenues reached a record high of $319.9 billion in 2015 (Megginson, 2017). While economists have made tremendous progress in understanding potential consequences of privatization, privatization decisions themselves remain a black box. In this paper, I study whether governments follow the principles of economic efficiency when deciding which firms they are going to privatize.

Assessing this question offers three major contributions to the understanding of privatization and government behavior. First, privatization decisions are an intriguing type of policymaking as benefits arising from it, such as sales revenues, are dispersed across the population, while potential costs (e.g. job losses) are highly concentrated in affected regions. This discrepancy allows the opponents of privatization to organize and campaign more effectively than its proponents. Thus, existing literature highlights the importance of political processes in the determination of privatization decisions (Roland, 2002). For instance, Dinc and Gupta (2011) find that governments privatize strategically to please voters in politically contested districts or which are in districts home to members of the government. The political motivation of privatization policies thus raises the question whether there is still a role to play for economic efficiency arguments in these decisions.

Second, firms' selection into privatization is non-random if governments sort more efficient firms into privatization programs. This type of selection would have a direct consequence for the empirical analysis of privatization outcomes. Brown et al. (2010) acknowledge that "the most difficult problem is the possibility of selection bias in the privatisation process". However, the literature on the effects of privatization mostly takes these decisions as exogenous, conditional on covariates, thereby relying on conditional independence assumptions (see e.g. Estrin et al., 2009, for a review). A profound analysis of privatization decisions helps to understand the selection of firms into privatization and examine the validity of these assumptions.

Third, I introduce the Treuhand firm surveys, a novel dataset which allows the

most comprehensive analysis of the German Treuhand privatizations. The Treuhand privatizations are one of the most ambitious privatization programs in history and have previously received little attention by researchers.

The Treuhandanstalt (short: Treuhand or THA) was a government agency founded in 1990, which became the owner of all state-owned enterprises of the German Democratic Republic (GDR). These enterprises comprised the GDR's entire industrial sector, among other businesses. Following German reunification, the Treuhand's task was to either transfer ownership to the private sector, or shut down enterprises if firms were nonviable.[1] This task had to be completed by 1994 but was complicated by the firms' lack of competitiveness. Analyzing classified data from the GDR's planning ministry, Akerlof et al. (1991) found that only one out of more than 400 examined companies was viable under world-market conditions. Hence, the choice between privatization and liquidation was highly ambiguous as the Treuhand had to offer substantial subsidies to potential buyers, commonly resulting in effectively negative sales prices. The dire need for subsidies left discretionary room for decision makers in determining which firms should be selected for privatization.

Post-reunification Germany is an ideal setting to study privatization decisions for at least five reasons. First, the privatization program itself was an exogenous and unanticipated event. Even expert observers did not foresee the fall of the Berlin wall in November 1989 and its potential consequences (Redding and Sturm, 2008; Burchardi and Hassan, 2013). Second, the program's massive scale, spanning more than 8,000 companies, is unusually large in a single-country context. The setting thus combines large-scale variation in firm characteristics with an otherwise institutionally homogeneous environment. Third, the law governing the program (TreuhG) did not specify precise privatization priorities. Instead, it defined multiple broad objectives, leaving plenty of opportunities for political agents to exert their influence. Fourth, direct sales to buyers was the main privatization method, rather than auctions or voucher programs where interference in the process is more difficult to attain. Fifth, privatizations in Germany occurred within a stable economic environment and followed a clear path of reform towards West German standards. Thus, privatization was credible in the sense of Perotti (1995), making future policy reversals unlikely. In sum, the Treuhand privatizations offer a unique opportunity to study large-scale privatization decisions within an almost ideal framework.

I start the analysis by discussing simple productivity-based rules, derived from economic theory, which describe the decisions of a government solely committed to economic efficiency. Such rules later provide a benchmark for analyzing observed behavior. I hypothesize that the economically efficient solution to the government's

[1] In fewer cases, the Treuhand also restituted firms to formerly expropriated owners or, for few specific industries such as public transport, transferred ownership to local municipalities. The federal government ruled out that it would retain ownership in any of the GDR's industrial companies.

decision problem is to privatize firms able to compete under market conditions and liquidate all others. However, in Germany, this would have translated into liquidating the entire industrial sector. Thus, to reduce undesirable side effects from liquidations, governments may set aside a budget to subsidize firms' privatization and ensure their future existence. In this case, the government should give support to those firms where subsidies can have the greatest effect, i.e. to the firms whose productivity is the highest and closest to subsistence. These rules provide empirically testable predictions, linking privatization decisions and firms' productivity.

Exploiting the first comprehensive data set on Treuhand-owned companies, I descriptively investigate whether the privatization decisions within the Treuhand program are consistent with these rules. The data stems from the Treuhand firm surveys, a panel survey which targeted the entire population of Treuhand firms between 1991 and 2003, providing unusually rich background information for participating firms. A complementary Treuhand firm census ensures the representativeness of the firms participating. Using these surveys, I construct several productivity performance indicators and match this data with additional regional and election data. Thereby, I am able to investigate alternative or complementary explanations for observed privatization patterns.

I find that privatization probabilities are increasing in firm productivity, consistent with the notion that governments take economic efficiency into consideration. This result is robust to conditioning on finely grained industry, state, and survey fixed-effects and consistently found for various productivity indicators. Notwithstanding, economic efficiency considerations explain less than 10% of the variance in linear models of privatization decisions. I do not detect that privatization decisions link with patronage, close elections, or potential lobby size. Hence, privatization decisions in Germany are likely influenced by economic rather than political factors.

The lessons are threefold. First, productivity matters for governments' privatization decisions – but it does not explain the full story. Second, a highly selective and complex process determines which public firms become private enterprises. Hence, any study of post-privatization effects needs to carefully consider a credible identification strategy to account for endogenous sorting of firms into privatization. Third, privatization decisions in Germany starkly differ from experiences in other countries where political factors are found to be highly influential (see, e.g. Gupta et al., 2008)). These different institutional settings and privatization decision processes may also rationalize the broad range of estimates for post-privatization firm performance across time and states.

Existing literature rarely studies privatization decisions themselves. For instance, the extensive review by Estrin et al. (2009) only lists the paper by Gupta et al. (2008). If the selection of firms into privatization is addressed at all, researchers

commonly emphasize political rather than economic factors as the determinants of these decisions. Moreover, they focus on the timing of privatization rather than the selection of firms into the program. Gupta et al. (2008), for example, study the sequencing in privatizations of 1470 firms in the Czech Republic. Similar to the theoretical model by Roland (1993), the authors find the timing of firm privatization to most closely relate to governments aiming at maximizing public goodwill. This implies that firms with higher current profitability are sold at earlier stages of the reform to maintain voter's support for further privatizations.

Dinc and Gupta (2011) study financial and political factors in the privatization decisions for 259 public non-financial companies in India, of which 49 were privatized between 1990 and 2004. The authors find profitable and low-wage-bill firms to be privatized earlier, consistent with the public goodwill argument. Moreover, governments delay privatizations of firms located in districts contested by opposition parties and never privatize firms in regions home to the minister in charge. While the analysis emphasizes that political competition and patronage matter for privatization decisions, it does not cover the role of economic efficiency and firm productivity.

Szentpèteri and Telegdy (2012) conclude for Romanian privatizations that governments sacrifice efficiency enhancements to minimize employment losses. In their analysis of 2019 state-owned firms, the authors find that politicians sheltered particularly large and inefficient firms even if they paid low wages and faced financial problems. De Fraja and Roberts (2009) study the privatization timing of 147 large firms in Poland. These sequencing decisions were most compatible with consumer surplus maximization motives rather than maximizing privatization revenues or minimizing employment losses. The authors do not consider political factors in their analysis. The political process of privatization though matters for potential voters. In a survey conducted in 28 post-communist countries, Denisova et al. (2012) find that citizens' view on privatization depends on the legitimacy they attach to the privatization proceedings. For Germany, Hau (1998) analyzes the decision-making of the German Treuhand based on 1,804 privatization and 1,097 liquidation decisions. Hau (1998) finds a subsidy bias towards large state-owned enterprises which was increasing over time, consistent with the interpretation of a growing political intervention risk over time.

Finally, this paper also builds upon the literature on the provision of local government services, such as water, energy, and waste management. These services can either be provided by local governments themselves, or contracted-out to private providers (Lopez-de Silanes et al., 1997). However, these sectors usually occur to be natural monopolies, making them a special case of privatization. After reviewing the existing papers on the motives for local privatization decisions, Bel and Fageda (2007) conclude that "any finding of significance for a particular relationship is

quite sensitive to the characteristics of the particular study", emphasizing the need for further research.

In the following section, I describe the institutional features of German privatization. Section 4.3 discusses theoretical foundations of government behavior in privatization decisions. Section 4.4 describes the data and descriptive statistics. The results are presented in section 4.5. I conclude in section 4.6 and highlight implications for policymakers as well as future research.

4.2 Institutional Background of the Treuhand Privatizations

This section briefly reviews the origins and main features of the German Treuhand privatizations. The fall of the Berlin Wall constituted a collapse of the GDR's centrally-planned and publicly-owned economy. As a consequence, the federal government of reunified Germany abolished the planning system, extending all existing West German market institutions and regulations to the East. The Treuhandgesetz (TreuhG) and preceding legislation of the last GDR government required that all state-owned enterprises, representing the GDR's entire industrial sector, were transferred to the newly created Treuhand agency. On July 1st 1990, the Treuhand effectively owned 10,334 companies with about 4 million employees. These firms composed all listings in the GDR's registry of publicly owned operations on July 1st 1990 (Kühl et al., 1991).

While other post-communist countries assigned the privatization task directly to a ministry, the Treuhand was a separate entity with a potentially larger degree of autonomy. However, the agency was subject to the supervision of the Federal Ministry of Finance. Its supervisory board was instated by the federal government, with members of all East German state governments directly represented on the board. The supervisory board instated the members of the executive board and controlled their decisions. Hence, political influences were likely through various formal and informal channels. Horst Siebert (1991), then a member of the German Council of Economic Experts, therefore warns that the "Treuhand operates under tight political scrutiny". The Treuhand created a two-tiered organizational structure, consisting of the Berlin-based headquarter and fifteen local branch offices. The headquarter was in charge of all enterprises with more than 1500 employees. Local branch offices were responsible for companies below this threshold. Furthermore, the Treuhand's firm database which allowed for a definite assignment was only available by April 1991. Despite the uniqueness of the East German setting, the institutional structure still resembles those found in other countries. In particular, the setup of the Treuhand Agency is comparable to the Hungarian State Property Agency (see Carlin and Mayer, 1994, p. 211).

The Treuhand's objective was to "privatize quickly, restructure resolutely, and liquidate carefully" (Bundesanstalt für Vereinigungsbedingte Sonderaufgaben, 2003). After assuming ownership of the GDR's state-owned enterprises, the Treuhand transformed them into companies under private law and split up large conglomerates into individual firms. The Treuhand then urged firms to compile opening balance sheets and business plans to find potential buyers. Few exceptions aside, the main method of privatization was direct sales to potential investors. This approach is in stark contrast to privatization programs in other transition countries where auctions and voucher systems facilitated sale processes and impeded external influences in privatization decisions.

Challenges inherent to the task complicated the Treuhand's efforts. Overstaffing, outdated production technologies, the loss of traditional trading partners from the Soviet Union, and a lack of market experience made East German firms a hard sell. Simultaneously, a currency reform and union bargaining put substantial upward-pressure on wages, further deteriorating the business situation (Dornbusch and Wolf, 1994). Akerlof et al. (1991) illustrate the extent of these problems by analyzing confidential data from the GDR's central-planning bureaucracy, covering the major business conglomerates. These conglomerates frequently exported products to clients outside the Soviet Union, so the authors combine domestic resource costs for these products and their respective export revenues in 1989 to calculate the expenses per revenue unit earned. The results present a dim picture of the firms' competitiveness, as all but one of 183 enterprises had production costs higher than their respective revenues. Consequently, the Treuhand was allowed to give significant concessions to potential buyers, including capital injections, debt redemptions, or the assumption of environmental liabilities. These subsidies commonly resulted in economically negative privatization prices (Hau, 1998).

A substantial organizational challenge was that the agency operated under two conflicting principles; intending to maximize profits through privatization but handling the task under the focus of social acceptability. Attempting to reconcile both goals, the Treuhand asked potential investors for employment and investment guarantees in addition to the sales price. Nevertheless, fierce protests by workers, unions, and local politicians accompanied numerous privatization and liquidation decisions.

The Treuhand closed its operations on December 31st, 1994. In total, 14,000 companies and company subsidiaries were privatized between 1990 and 1994, preserving 1.5 million out of 4 million jobs. The financial loss from its operations amounted to 230 billion Euros. The Bundesanstalt für vereinigungsbedingte Sonderaufgaben (BvS) took over remaining tasks, such as contract surveillance. Although the Treuhand was dissolved many years ago, the consequences of its decision are still present in today's East Germany (Goschler and Böick, 2017).

4.3 Economic Efficiency and Privatization Decisions

When evaluating their portfolios of publicly-owned firms, governments have to choose which firms to privatize, which ones to liquidate, and which ones to maintain under public ownership. This section asks how governments should make their decisions according to economic efficiency and alternative goals. Existing economic theory typically uses one of two opposing assumptions regarding the behavior of governments in privatization decisions. The first group assumes rational welfare-maximizing governments, where welfare is typically the combination of consumer and producer surplus. This assumption is typical for models in the incomplete contracts literature on privatization, including Laffont and Tirole (1991), Schmidt (1996), or Lülfesmann (2007). Inefficiencies in these models do not stem from the government's behavior but from asymmetric information between the government and the firm's manager, resulting in principal-agent problems.

The second stream of models assumes egoistic governments aiming to increase their personal benefits and, in particular, their reelection chances. These assumptions are common in papers within the public choice tradition, such as Biais and Perotti (2002). Roland (1993, 1994) argues that privatization policies are subject to a structural mismatch of political costs and benefits. The political costs of shutting down an inefficient enterprise are concentrated primarily in workers whose jobs are in danger. Conversely, the respective gains are widespread across the population through the lower need for public subsidies. This mismatch incentivizes vote-seeking politicians to dismiss economic efficiency and bail-out nonviable firms. In this spirit, Shleifer and Vishny (1994) and Boycko et al. (1996) assume that politicians derive utility from (excess) employment. By disregarding costs for the public, governments of this type directly violate economic efficiency.

In the case of East Germany, the federal government ruled out maintaining state ownership at all, effectively shrinking its choice set to privatization or liquidation. Combining both strands of the literature and applying them to the German setting, I may generalize the government's problem as

$$Privatization_i = f(Economics, Politics)$$

where the decision to privatize firm i is a function of economic and political considerations. The strength of these factors depends on the specific objectives pursued by the government.

A government purely committed to economic efficiency is a natural benchmark. How should it decide between privatization and liquidation within a given portfolio of firms? Assuming that the ability of a state-owned company to survive under market conditions is best described by its productivity, governments should save

those firms from liquidation whose productivity is equal to or above the productivity levels in a competitive market. These firms do not require state subsidies and should sell at non-negative prices. Firms with lower productivities, and thereby lower sales values, should then be liquidated. Otherwise, governments would provide costly bail-outs to nonviable companies. Hence, the government could rank all firms according to their productivity and privatize those above the competitive market threshold. As a minimum criterion for an efficiency-oriented agenda, I presume that more productive firms have a higher probability of privatization than low-productivity firms. In particular, I expect this pattern to hold for firms within the same industry, as they face more similar competitive and regulatory environments.

Additional alterations are necessary to adapt these considerations to the German context. This is because hardly any East German firm was viable under market conditions, as found by Akerlof et al. (1991) and explained in section 4.2. Fearing a de-industrialization of East Germany, the government granted the Treuhand additional means of financing to cover these losses and subsidize privatizations. For instance, in 1991, the Treuhand had a financial framework of DM 25 bn and an additional loan guarantee limit of DM 35 bn at its disposal (Christ and De Weck, 1991).[2] Given the privatization budget, a government committed to economic efficiency would still privatize the most competitive firms. However, the cutoff productivity at which a firm is privatized now decreases by the productivity value of the public sales subsidy. This enables a larger number of firms to avoid liquidation. To maximize the subsidy budget's impact, such a policy should support those firms that are closest to the productivity frontier. Consequently, the previous rule that more productive firms have a higher rate of privatization should still hold true, particularly within industries.

In contrast to efficiency-oriented governments, existing literature highlights political factors determining privatization decisions. In particular, employees losing their jobs are directly linked and thus can easily form an effective lobby group. Conversely, beneficiaries of privatizations are more widespread and thus face difficulties in organizing and campaigning. Governments susceptible to lobbying will thus always try to prevent liquidations. This effect should be particularly strong for large firms and firms that are an important employer in their region, another testable implication for privatization decisions. Similarly, the effect should be stronger for districts that are highly contested in elections.

Finally, political pressures may give rise to particular time patterns of privatization. If the hypothesis embedded in the theoretical model by Hau (1998) is correct, political pressure for the Treuhand was growing over time as the opposition to liquidations had to organize their lobby first. This implies that the role of productivity

[2]The official exchange rate set for the Euro's introduction is 1 DM = 0.51 EUR.

in privatization decisions should have decreased over time while political factors should have increased.

4.4 Data

The Treuhand Firm Surveys are the foundations of this analysis and augmented with additional data sources. The Treuhand Firm Surveys covered all firms currently or formerly owned by the Treuhand. Starting in April 1991, these firms received questionnaires every six months which were administered by the Berlin-based SOESTRA institute (Kühl et al., 1991). From October 1994 to 2003, surveys were conducted annually. Formally, participation was not obligatory. However, an official cover letter by the head of the Treuhand asked firms for a reply.

The sampling frame comprises the universe of Treuhand companies from the Treuhand's administrative records. Based on the Treuhand's adminstrative records, the survey data contains basic background information for all firms, including four-digit industry identifiers, district of firm location, privatization status, and the month when Treuhand ownership ended. Firms' survey responses yield additional items. Questionnaires consistently covered employment and revenue items, while other questions varied among survey waves. Moreover, response rates between 20 to 65% (with higher response rates in earlier surveys) implied that not every item is available for every firm in every wave. For more than 74% of all firms, at least one valid response to a survey wave is available. Appendix C.4 presents further background information on the data in addition to evidence on its validity and representativity.

Consistent with the privatization literature (see, e.g. Frydman et al., 1999; Brown et al., 2006, 2010), I focus my analysis on industrial companies and drop firms from the service and agricultural sector from the original sample of 14,961 firms. I also exclude municipalized and firms restituted to previous owners. Municipalities became the owners of Treuhand firms only in pre-determined economic activities related to communal functions, mainly in the energy and public transport industries. Restitutions were granted based on audited requests by formerly expropriated owners and followed strict guidelines. Hence, both decision procedures were largely outside the government's direct control. Applying these boundary conditions results in a final sample of 6,338 firms.

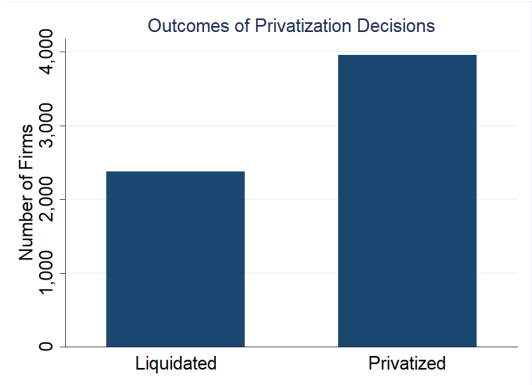
Addressing varying question types and response behaviors across different waves, I collapse all surveys into a cross-sectional dataset and always keep the earliest response to a given question. For instance, for the employment variable, this procedure implies that the item is available for 5,315 firms (84%), of which 4,109 (77%) stem from 1991, and the remainder from later waves. I apply a 1% winsorizing rule to financial variables as they may be prone to measurement error

Table 4.1: Descriptive Statistics

	Mean	Std. Dev.
Privatization (=1 if privatized at the end of 1994)	0.62	0.49
Annual Revenue per Worker (in 1000 DM)	111.16	186.56
Revenue per Hour Worked (in DM)	59.95	98.08
Gross Value Added per Year (in 1000 DM)	9873.86	24746.56
Share of Employees Working in R&D (%)	2.82	7.24
Vote share of government coalition, second vote (%)	56.79	7.13
Abs. vote share difference, second vote (%)	21.32	11.23
Employee Share Within State (%)	0.07	0.18
Wage Bill Share Within State (%)	0.07	0.17

Notes.– Treuhand Firm Data, own computations.

Figure 4.1: Overall Frequency of Privatization



(Bollinger and Chandra, 2005). Employment variables based on simple counts are more easily measurable and therefore remain unaltered.

My main variable of interest is the privatization status of a firm in December 1994, when the Treuhand finished its operations. This dummy variable is one if a firm was privatized and zero if it underwent liquidation. Figure 4.1 shows that privatized firms account for roughly two-thirds of all firms in my sample. The main covariate is productivity which I operationalize in three different ways. The first one is the ratio of total revenues to the total number of employees, as frequently used in the privatization literature (see Estrin et al., 2009, p. 720, for a comprehensive review). Secondly, I construct revenues per hour, which additionally accounts for the number of employees in short-time work. Short-time work is a government-sponsored wage subsidy program for East German firms aiming to reduce hours of work (Dornbusch et al., 1992). The third concept is gross-value added, which excludes expenses for raw, auxiliary, and operating materials from total revenues. The latter measure is only available from surveys in October 1993 and 1994, which is why it is only used as a supplementary criterion.

The profit variable is defined as earnings before taxes, interests, and depreciation (EBITDA). These are the total revenues minus the costs for material and labor

inputs. Financial variables are generally measured in German Mark (DM) and price-adjusted to the baseline year 2000. Further firm characteristics are directly retrievable from the Treuhand Firm Surveys. Table 4.1 provides summary statistics for each variable.

For supplementary analyses, I match the firm surveys with further industry-level information. These include the productivity levels of East German industries relative to their West German counterpart in 1990. Heske (2014) provides this aggregated data for 14 industries. Its reliability confirmed by Glitz and Meyersson (2017), the data provides a rare measure of the GDR's productivity distance to the West German frontier. Moreover, I add the OECD's classification of technology-intensive sectors (Hatzichronoglou, 1997).

To test for political channels, I complement the firm information with vote counts per party from the first national election in reunified Germany, in 1990, from Bundeswahlleiter (2016). I match vote counts from electoral districts with firms' municipality to compute local vote shares for the governing party, the governing coalition, as well as the difference in vote shares between the governing coalition and opposition parties.

4.5 Empirical Approach and Results

4.5.1 Empirical Approach

This paper takes a descriptive approach to explore the role of economic efficiency for privatization decisions. I employ binned scatter plots to assess the relationship between the firms' privatization status and productivity, the main explanatory category of interest. The binned scatter plots divide all observations into twenty equally sized groups according to the explanatory variable and compute the mean privatization share for each bin. This strategy offers two major advantages. First, the approach does not rely on potentially implausible parametric assumptions. Second, it is intuitive and easy to understand. The scatter plots additionally feature a linear fit curve through the underlying data points, indicating the direction of a potential correlation.

Simple linear probability models complement the semi-parametric analysis to account for potentially confounding factors and get a sense of the correlations' strength. I therefore employ estimation equations of the form

$$Privatization_i = \delta Productivity_i + \beta_2 IndustryFE_i + \beta_3 StateFE_i + \beta_4 SurveyFE_i + \varepsilon_i \tag{4.1}$$

where $Privatization_i$ denotes the privatization status of firm i at the end of the year 1994 (dummy). Most importantly, I regress the privatization status on a firm productivity indicator, with δ being my coefficient of interest. The regression in-

cludes industry fixed effects at the four-digit level to account for general industry differences and for public support programs which may have benefited particular industries. On average, this leaves about 180 firms per industry cell. State fixed effects control for state-level policies and geographic heterogeneity. Finally, survey fixed effects for the year and month of the first appearance within the firm survey responses capture additional seasonal, macroeconomic, and survey-specific factors. Estimating the equation by ordinary least squares yields the best linear approximation of the respective conditional expectation function. The underlying assumption is that the covariates are the only reason why $Productivity_i$ and ε_i are correlated. In the appendix, I confirm the main results using the respective logit models of equation 4.1. I use Eicker-Huber-White standard errors throughout as a safeguard against heteroscedasticity.

After analyzing the role of productivity for privatization decisions, I assess factors that may contribute to both, higher privatization probabilities and higher productivity. Finally, I analyze alternative explanations for observed privatization patterns with a special focus on political factors.

4.5.2 Privatization - An Overview

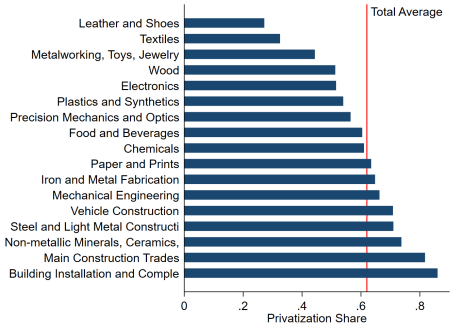


Figure 4.2: Privatization by Industry

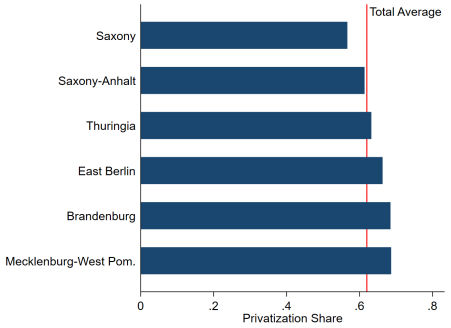
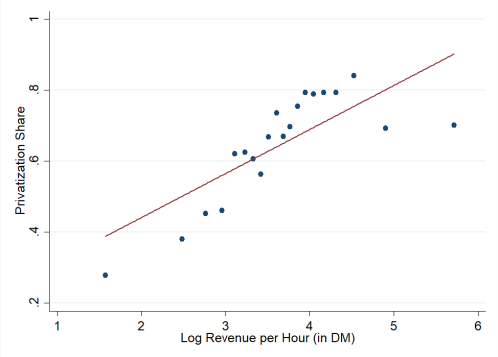


Figure 4.3: Privatization by State

Figures 4.2 and 4.3 present the privatization shares by industry and state. There is considerable variation in privatization decisions across industries. The building, construction, and non-metallic minerals industries experienced particularly high shares of privatization. In contrast, industries producing leather and shoes, textiles, or simple metalworkings faced liquidations in more than half of all cases. Privatization shares differ less across East German states, although the figure is slightly lower in Saxony. A potential explanation is that the leather and textiles industry which fared particularly badly is more concentrated in Saxony.

Note: Dots represent binned scatter plot where the observations are grouped into twenty equally sized bins. A simple linear regression line indicates the direction of the correlation.

Figure 4.4: Privatization and Revenues per Worker



Note: Dots represent binned scatter plot where the observations are grouped into twenty equally sized bins. A simple linear regression line indicates the direction of the correlation.

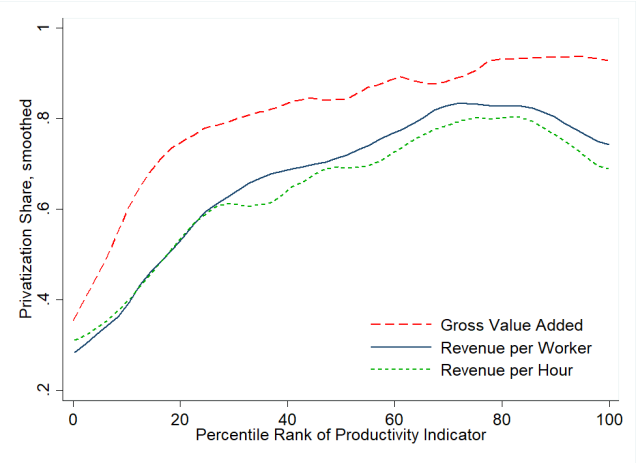
Figure 4.5: Privatization and Revenues per Hour

4.5.3 Privatization Decisions and Firm Productivity

This section presents the results of my empirical assessment on the link between productivity and selection into privatization using two alternative productivity indicators, revenues per worker and revenues per hour worked. I transform the productivity variables using the natural logarithm. Applying these transformations allows for non-linear relationships between productivity and privatization, symmetrizes the otherwise highly skewed variables, and ensures a percentage-point interpretation of respective regression coefficients.

Figure 4.4 and 4.5 present binned scatter plots for the relationship between productivity and the share of privatized firms. They indicate that privatization shares are increasing in both, revenues per worker and revenues per hour. Firms at the bottom 10% of the productivity distribution were privatized in less than 40% of all cases according to both measures. In contrary, privatized occurred in more than 65% for firms in the top 10% of the productivity distribution. These patterns strongly support the role of productivity in privatization decisions.

Ideally, the productivity variables describe the pre-privatization situation to avoid the scenario that productivity has changed as a result of privatization. Hence, the variables need to be measured before the privatization process started, which was in 1991 and thereafter for most firms. Figures C.1 and C.2 therefore repeat the previous analysis but restrict the sample to firms participating in a 1991 survey and still in Treuhand ownership. The resulting binned scatter plots for revenue per worker and revenue per hour show a strong positive relationship between productivity and privatization chances. They are also remarkably similar to the previous graph, showing that the timing of measurement likely does not impede the analysis.



Note: Rank coefficients calculated using Hazen's rule. Smoothing procedure uses an Epanechnikov kernel function of degree zero.

Figure 4.6: Non-Parametric Analysis Using Percentiles

Another objection to the previous result may suggest that revenues per worker and revenues per hour do not capture differences in the amount of inputs used in firms' production process. I thus repeat the analysis using gross value added as an alternative productivity indicator. Figure C.3 in the appendix shows that productivity-privatization nexus found above consistently extends to this measure. Figure 4.6 repeats the plots of privatization and productivity, using percentile ranks as a non-parametric safeguard against potential outliers. This graph confirms that privatization increases in productivity, with the exception of very high productivity ranks.

Table 4.2: Regression Results: Privatization and Productivity

	(1) W/o Controls	(2) Industry Dummies	(3) State Dummies	(4) Survey Dummies
Revenue per worker	0.137***	0.117***	0.116***	0.107***
	(0.007)	(0.009)	(0.009)	(0.009)
Industry FE	-	✓	✓	✓
State FE	-	-	✓	✓
Survey FE	-	-	-	✓
R-squared	0.081	0.199	0.203	0.209
Observations	4,655	4,655	4,655	4,655

Notes.– * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. Each column presents a different estimation of equation (4.1). The outcome variable is a dummy equaling 1 if a firm was privatized at the end of 1994 and 0 otherwise. Revenue per worker is in logs. Robust standard errors given in parentheses.

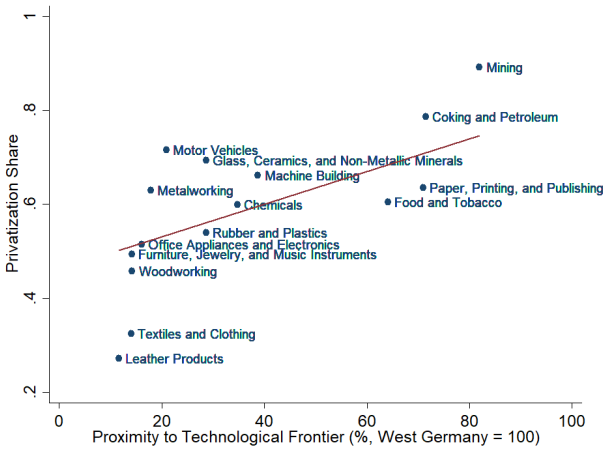
Finally, I evaluate whether the main results hold true after controlling for poten-

tially confounding factors. Table 4.2 presents the results of the linear probability model for our main productivity indicator, revenues per worker. Each column represents a separate estimation of equation (4.1). The first column estimates the relationship without any further controls, suggesting that a 1% increase in revenues per worker is associated with a 0.14 percentage point (p.p.) increase in the privatization probability. This correlation remains remarkably stable in size and significance even after subsequently controlling for additional factors. Column 2 introduces a full set of four-digit industry dummies, meaning that the privatization-productivity relationship is only inferred from within-industry variation. Column 3 additionally includes state dummies, and column 4 presents results after including survey dummies. Even after controlling for these factors, a 1% increase in productivity still increases privatization probabilities by 0.1 p.p.. Table C.1 in the appendix repeats the final estimation for the alternative productivity indicators and confirm the previous results. Table C.2, also in the appendix, repeats the same exercise but using a logit estimation, corroborating the signs and significances in the previous table.

Assessing the explanatory power of productivity, the coefficient of determination of the purely productivity-based model only ranges at around 8%. Still, the same simple linear specification is able to predict 71% of all privatization decisions correctly.[3] Hence, productivity is a major predictor of privatization decision but does not explain the entire variation in observed privatization patterns, motivating the search for further determinants.

4.5.4 Potential for Productivity Increases

[3]To do so, I randomly split the observations into an estimation and validation sample. I run specification (1) of Table 4.2 on the estimation sample and then predict the outcomes in the validation sample. I classify firms with predictions above 0.5 as privatized and liquidated otherwise. Comparing the predicted with the actual outcomes, the proportion of correct predictions then is 71%.

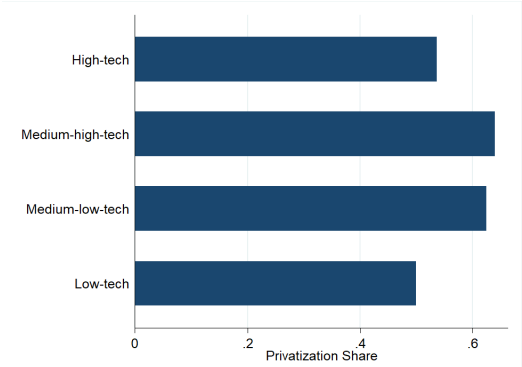


Note: Industry-level analysis using the classification by (Heske, 2014). A simple linear regression line indicates the direction of the correlation.

Figure 4.7: Privatization and Closeness to the Technological Frontier

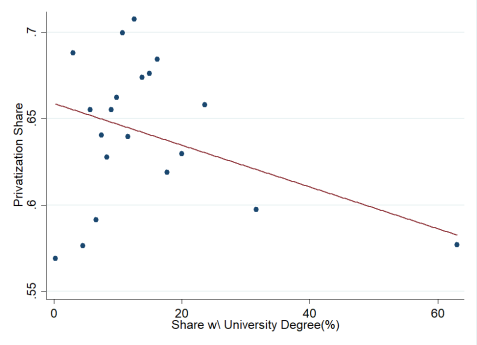
Absolute productivity measures, as analyzed in the previous paragraphs, may be uninformative about the potential productivity a firm could achieve by implementing the latest technologies. Next, I thus analyze whether proximity to the West German frontier at the industry-level in 1990 was relevant for firms' privatization chances. West Germany provides a useful benchmark of the technological frontier as it was a competitive market economy highly integrated into global production chains. Repeating the previous graphical analysis, Figure 4.7 reveals that privatization probabilities increase in proximity to the technological frontier. This finding highlights that firms from industries with initially better productive conditions were more likely to be privatized. I perform a similar analysis using a broader range of industries classified according to the international OECD categories of technology-intensive sectors from (Hatzichronoglou, 1997). From this perspective, low-technology industries have the lowest and medium-high-tech the highest privatization chances.

An educated workforce and a strong R&D department provide further potential for future productivity increases, making them considerable factors for privatization decisions. Figure 4.9 presents the respective binned scatter plot for the share of employees with a university degree. Surprisingly, the share of highly-educated employees is not increasing with privatization shares but even decreasing. At least three reasons may explain this finding. The finding could stem from industry effects, a low value of university degrees in East Germany (e.g. due to the politicization of university admissions), or from buyers being only interested in acquiring production capacities, which may not require high-skilled workers. Figure 4.10 plots the same graph for the share of employees working in R&D. Underpinning a



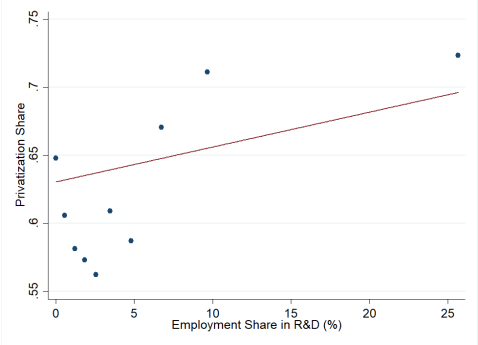
Note: OECD industry classification of technology-intensive sectors.

Figure 4.8: Privatization and Sectors' Technology-intensity



Note: Dots represent binned scatter plot where the observations are grouped into 20 equally sized bins. A simple linear regression line indicates the direction of the correlation.

Figure 4.9: Privatization and Employee Qualification



Note: Dots represent binned scatter plot where the observations are grouped into 20 equally sized bins. A simple linear regression line indicates the direction of the correlation.

Figure 4.10: Privatization and R&D Intensity

positive relationship, the graph reveals that companies with more than one-tenth of employees working in R&D exhibit privatization probabilities above 70%. This finding speaks against the argument that investor demand would disregard firms which comprise more than plain production capacities. Higher R&D personnel shares are more closely associated with privatization than larger numbers of university graduates in the workforce, potentially underlining the option value of R&D personnel for future productivity increases. Table C.3 in the appendix complements the graphical analysis with respective OLS regressions according which control for general industry differences, among others. The results support the claim that R&D personnel but not employee education is associated with privatization decisions.

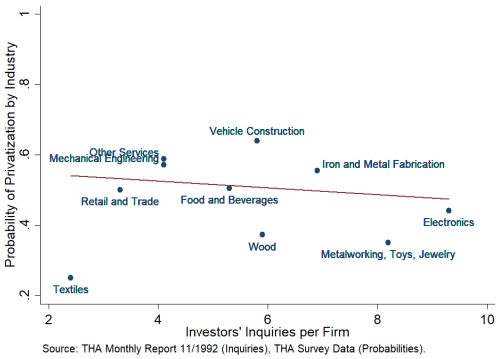


Figure 4.11: Privatization and Investor Inquiries

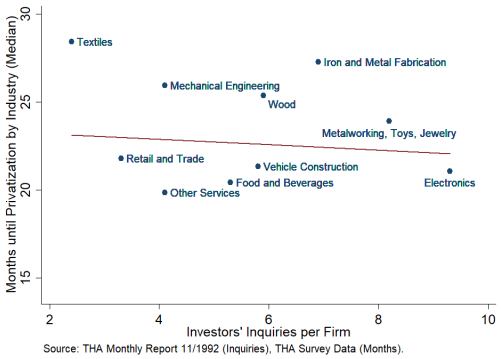


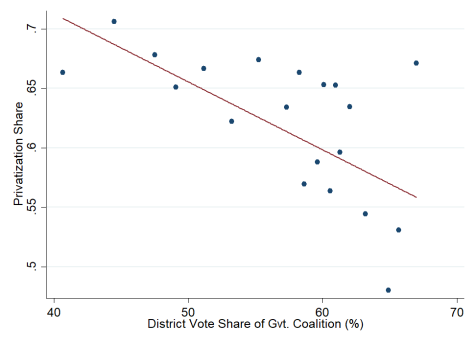
Figure 4.12: Months Until Privatization and Investor Inquiries

4.5.5 The Role of Investor Demand

The government and the Treuhand might have had little impact on the decision between privatization and liquidation as privatization required the availability of a potential investor. Figures 4.11 and 4.12 are based on industry-level data from the Treuhand on the number of investor inquiries per firm. To inspect firms' confidential financial data, potential investors had to formally request this information, which is reflected in this indicator. The resulting figures do not suggest any correlation between investor requests and privatization probabilities as well as the months until privatization (for the subsample of privatized firms). Thus, firm demand from outside is neither a predictor of privatization probability, nor of the time of privatization. Although inquiries are not a perfect indicator of investor interest, the analysis suggests that there is room for Treuhand- and government-based explanations of privatization behavior. This is particularly understandable given the necessity of subsidies and concessions for any kind of firm sale. Thus, the availability is endogenous and relies upon efforts and concessions made by the privatizing institution.

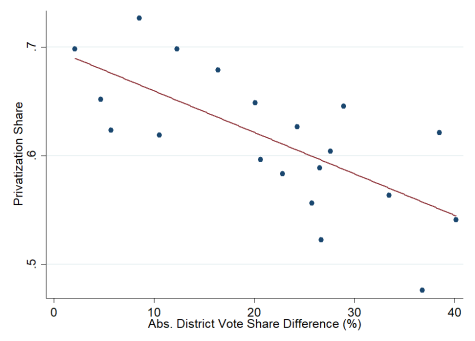
4.5.6 The Role of Electoral Competition and Lobby Groups

With productivity likely explaining a significant but rather small part of privatization decisions, additional factors must contribute to the patterns observed. Following the literature, the prime explanation for privatization decisions is politics. Political explanations stem from two different channels. First, (re)election concerns based on previous election outcomes may drive politicians' behavior and make them seek influence in privatization decisions to reach higher rates of firm survival for their electorate. Second, people and organizations (e.g. unions) affected by privatization decisions may form lobby groups, campaigning against liquidations and labor shedding. As a consequence, politicians could also resort to take influence



Note: Dots represent binned scatter plot where the observations are grouped into 20 equally sized bins. A simple linear regression line indicates the direction of the correlation.

Figure 4.13: Privatization and Political Patronage



Note: Dots represent binned scatter plot where the observations are grouped into 20 equally sized bins. A simple linear regression line indicates the direction of the correlation.

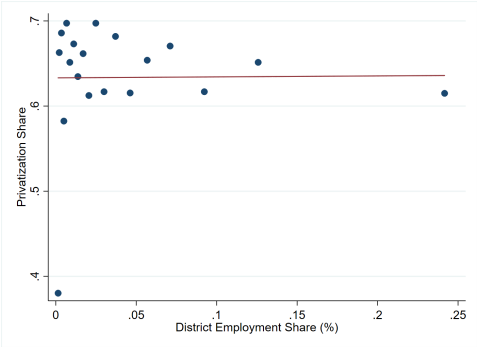
Figure 4.14: Privatization and Electoral Competition

and please these constituencies.[4]

Within the election channel, incumbent politicians may be particularly wary of liquidations in districts where the previous election outcome was particularly tight. As described in section 4.2, formal and informal ways were available to exert influence. Alternatively, politicians could use political patronage to reward their core electorates by preventing liquidations in these districts. The following analysis addresses both types of election channels.

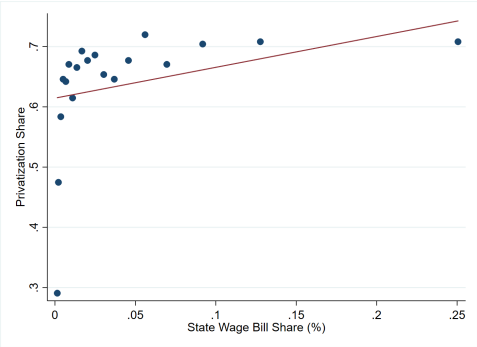
Figure 4.13 shows binned scatter plots for the privatization share and district vote share of the government coalition at the federal election 1990. It reveals that privatization shares strongly decrease with higher government votes, a finding which contradicts the political patronage argument. Figure 4.14 repeats the analysis for the vote share difference between government and opposition parties as absolute values, which indicates the outcome tightness of the previous federal election. With privatization shares decreasing in the vote share difference, it appears that intense electoral competition induces incumbents to fight for privatizations and avoid liquidations. However, further analysis unveils that this conclusion is premature as a simultaneous correlation with productivity exists. Figures C.4 and C.5 in the appendix repeat the analysis of the election channel but now plotting productivity on the y-axis. Both, the vote share of the governing coalition and electoral competition intensity decrease in productivity. Therefore, we can equally conclude that productivity, rather than election factors, are the driving force behind privatization decisions. Adding the two election channel variables to the main specification, the regression results in Table **??** support this argument. The Table provides results for repeated estimations of equation (1) but now taking political factors into account.

[4]Moreover, political ideology my drive privatization decisions. The topic is beyond the scope of this paper but briefly discussed in Appendix C.3.



Note: Dots represent binned scatter plot where the observations are grouped into 20 equally sized bins. A simple linear regression line indicates the direction of the correlation.

Figure 4.15: Privatization and State Employment Shares



Note: Dots represent binned scatter plot where the observations are grouped into 20 equally sized bins. A simple linear regression line indicates the direction of the correlation.

Figure 4.16: Privatization and State Wage Bill Shares

It turns out that productivity remains a strong predictor of privatization, whereas the election variables in column (1) and (2) are indistinguishable from zero.

Regarding the lobby group channel, I analyze two main measures for the potential lobby strength of a company's workforce. The first one is the employment share within a given state as a relative measure of the firm's relevance as a regional employer. The state-level relationship may be particularly crucial as state governments are directly represented in the Treuhand's supervisory board and thus possess an influential position. The resulting Figure 4.15 does not indicate a relationship between privatization and the state employment share. The second one is the state wage bill share of a firm to additionally reflect the wage level as an indicator of the jobs' value. Regarding the wage bill share, Figure 4.16 documents moderately increasing privatization shares for larger potential lobby group strength. Hence, the graphs only provide a weak foundation for the claim that a larger relative number of jobs increase firm survival. Both plotted share measures are winsorized at the 5% and 95% level to safeguard the analysis against potential outliers. Figures C.6 and C.7 repeat the graphical analysis using original variables to document that the interpretation does not rely on my handling of extreme values. The conclusions from these graphs are complicated by the observation that the variable distinguishing employment and the wage bill share, average gross wages, is also positively correlated with productivity (appendix, Figure C.8). Hence, higher wage bills due to higher productivities are an alternative explanation for greater privatization shares. Columns (3) and (4) of Table **??** provide the respective regression results, supporting the case that productivity rather than lobby group sizes mattered for privatization decisions.

Table 4.3: Regression Results: Privatization and Politics

	(1)	(2)	(3)	(4)
Revenue per worker	0.106***	0.106***	0.107***	0.106***
	(0.009)	(0.009)	(0.009)	(0.009)
Employment Share	-0.043			
	(0.035)			
Government Vote Share			0.002	
			(0.002)	
Abs. Vote Share Difference				0.000
				(0.001)
Industry FE	✓	✓	✓	✓
State FE	✓	✓	✓	✓
Survey FE	✓	✓	✓	✓
R-squared	0.205	0.205	0.205	0.205
Observations	4,572	4,572	4,572	4,572

Notes.– * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. Each column presents a different estimation of equation (4.1). The outcome variable is a dummy equaling 1 if a firm was privatized at the end of 1994 and 0 otherwise. Revenue per worker is in logs. Lower sample sizes in comparison to Table 4.2 due to item non-response and electoral district reforms. Robust standard errors given in parentheses.

4.6 Conclusion

This paper provides one of the first analyses exploring how governments select firms for privatization. I employ novel data on 6,433 formerly state-owned firms in one of the world's most ambitious privatization programs, initiated by the German government following the collapse of the Berlin Wall. I find that productivity plays a vital role in governments' privatization decisions. Political factors including close elections and the strength of potential lobby groups do not receive empirical support, contrary to their importance in the existing literature.

The results support economic efficiency as a motive for governments' privatization decisions, although it is only able to explain a minor share of the total variation. Also, the findings do not imply that governments promote economic efficiency beyond the selection of firms for privatizations. Political forces may still drive privatizations in ways not considered within this paper or not observable to researchers. It is also possible that political factors did not matter in deciding on whether to privatize or not, but did play a role in the decision about the extent of subsidies and other concessions granted to investors – a question beyond the scope of this paper.

More generally, the analysis demonstrates that the selection of firms into privatization is endogenous. In particular, it may not only be endogenous with respect to political factors, but certainly is endogenous with respect to initial firm productivity. Thus, analyzing privatization outcomes necessitates convincing identification strategies. However, the results caution against the use of close elections as an instrument for regional privatization levels (Dinc and Gupta, 2011), since local firm productivity levels potentially correlate with election results.

A major qualification requires further consideration. The institutional environment certainly plays a crucial role in the formation of selection procedures and privatization outcomes. Such differences in institutions likely explain different findings in the study of Indian privatizations by Dinc and Gupta (2011). In return, these differences may provide a valuable opportunity to better explain the widely varying effects found in the sizable literature on the impact of privatization for firms. Future work should try to explore these heterogeneities further, acquire a deeper understanding of privatization decisions, and link these insights to firms' post-privatization outcomes.

A Appendix to Chapter 2: Public Employment Services Under Decentralization

A.1 Description of variables

Variable	Description
FEA data	
Monthly job-finding	No. of monthly outflows of unemployed job center clients into employment
Monthly job-finding rate	No. of job-findings divided by the stock of unemployed
Vacancies, inflow	No. of newly registered vacancies registered by the FEA within month
Vacancies, stock	No. of registered vacancies registered by FEA
Unemployed, inflow	No. of newly registered unemployed job center clients within month
Unemployed, stock	No. of unemployed job center clients
Share: Unemployed <25 yr	Share younger than 25 years (15–24 years)
Share: Unemployed >55 yr	Share older than 55 years (55–64 years)
Share: Foreign nationals	Share of foreign nationals
Flow into ALMP	No. of monthly outflows of unemployed job center clients into any ALMP
Into short-term training	Outflows into short-term training
Into subsidized employment	Outflows into subsidized employment (wage subsidies)
Into medium-term training	Outflows into medium-term training (>three months)
Into public job creation schemes	Outflows into job creation schemes

continues on next page

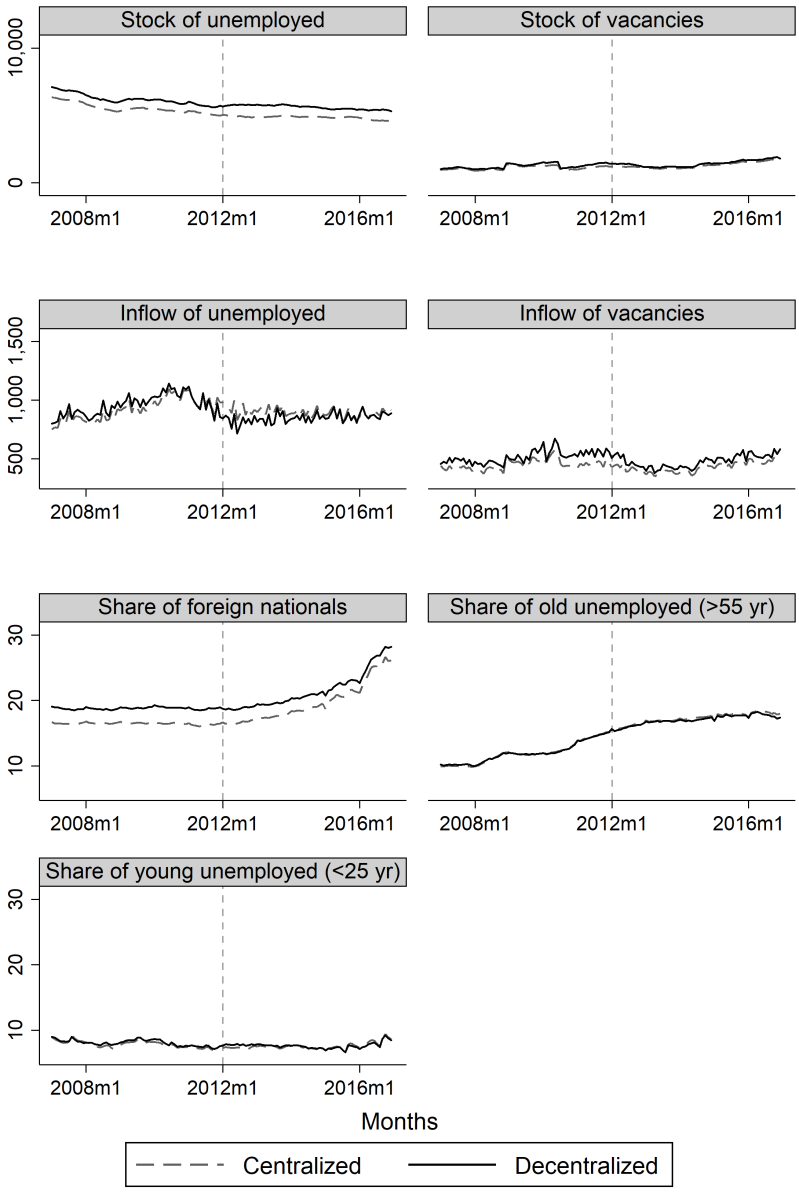
Monthly flow rate into ALMP	No. of outflows into any ALMP divided by stock of unemployed
New sanctions	No. of new sanctions issued on job center clients within month
Monthly sanctioning rate	No. of new sanctions issued divided by stock of unemployed
Stock of sanctions	No. of active sanctions for job center clients
Stock of benefit sanctions	Sanctions which reduce welfare benefits
Stock of accommodation sanction	Sanctions which reduce accommodation benefits
Outflow out of welfare	No. of outflows out of welfare receipt
Permanent outflow out of welfare	Outflows lasting for at least three months
Share: Permanent outflow	Outflow share lasting for at least three months
Destatis data	
GDP per capita (in 1,000 euros)	Gross domestic product per capita
Public debt p.c. (in 1,000 euros)	Municipal debt per capita
Urban district (dummy)	Urban district type (German: *Stadtkreis*)
East Germany (dummy)	East Germany (former GDR including Berlin)
Civil labor force (in 1,000)	No. of persons in civilian employment plus registered unemployed
Job-center unemployment rate	No. of unemployed job center clients divided by civil labor force
Employment rate	No. of persons in civilian employment divided by civil labor force
Share: Agriculture	Employee share in agriculture
Share: Mining and energy	Employee share in mining and energy
Share: Manufacturing	Employee share in manufacturing
Share: Construction	Employee share in construction
Share: Trade, transp., comm.	Employee share in trade, transport, and communications
Share: Finance and real estate	Employee share in finance and real estate
Share: Public and priv. services	Employee share in public and private services

Notes.– FEA: Federal employment agency. Destatis: German Statistical Office.

Table A.1: Table of Variables

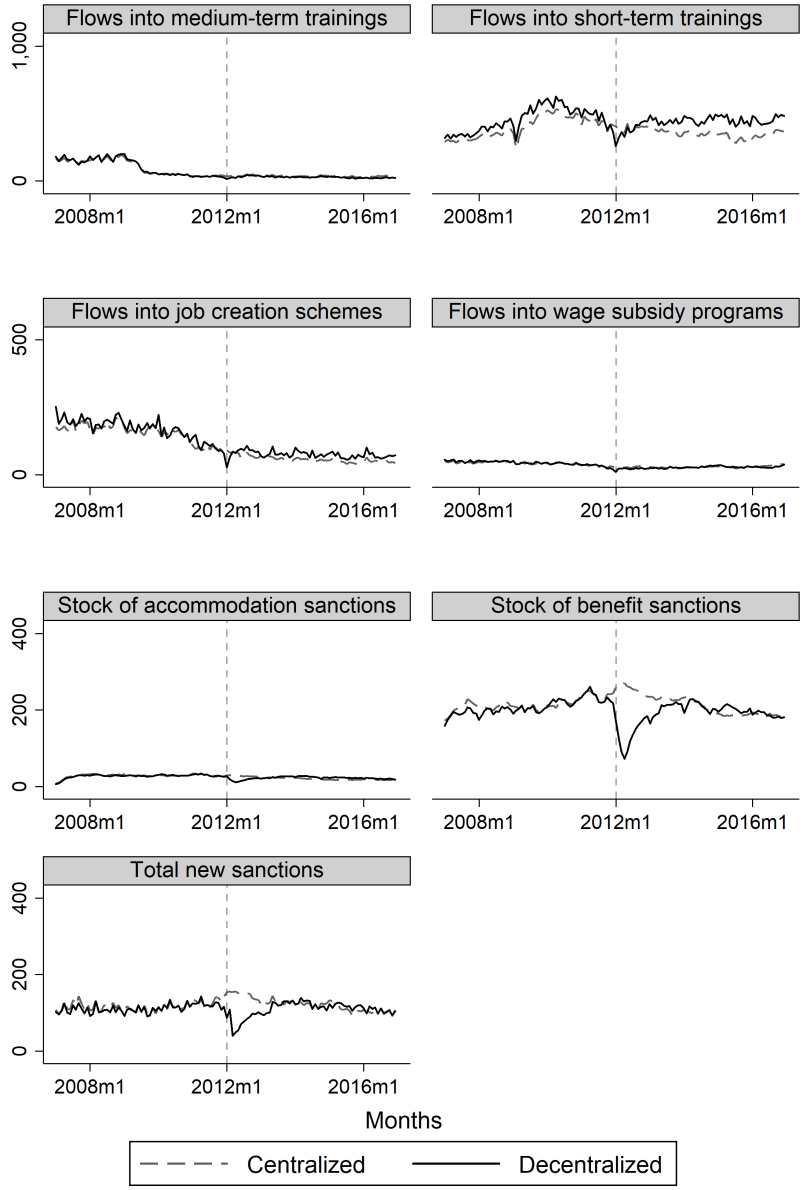
A.2 Additional Descriptive Graphs

Figure A.1: District Characteristics Over Time by Job Center Type (Part I)



Notes.– The upper four panels depict monthly stocks and inflows of vacancies and unemployed for districts in our sample. The lower three panels depict the demographic composition of the unemployed.

Figure A.2: District Characteristics Over Time by Job Center Type (Part II)



Notes.– The upper four panels depict monthly inflows into almp measures for districts in our sample. The lower three panels depict the stocks of sanctions in place by sanction type and the total inflow of new sanctions per month.

A.3 Time Trends and Alternative Sample Periods

Table A.2 adds linear time trends to the baseline model specified in equation 2.1 while Table A.3 varies the sample period to ensure our results are not driven by the transition period around the reform's implementation.

Table A.2: Difference-in-Differences: Adding Linear Time Trends to the Baseline Model

	(1) East trend	(2) State trends	(3) District trends
Decentralized	-0.097***	-0.112***	-0.093***
	(0.021)	(0.019)	(0.033)
R-squared	0.999	0.999	0.999
Districts	334	334	334
Observations	39,018	39,018	39,018

Notes.– * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. Each column presents a different estimation of equation 1. *Decentralized* is a dummy equaling 1 for districts with decentralized job centers and 0 otherwise. All continuous variables in logs. Regressions include a full set of dummies for districts and months. Standard errors given in parentheses are clustered at the job center and month level.

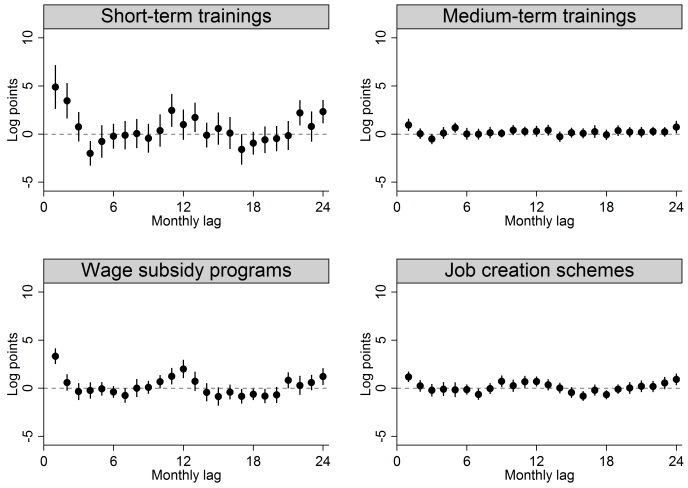
Table A.3: Difference-in-Differences: Alternative Sample Periods

	(1) W/o 2011	(2) W/o 2012	(3) W/o 2011 & 2012
Decentralized	-0.099***	-0.085***	-0.088***
	(0.021)	(0.021)	(0.022)
R-squared	0.958	0.959	0.958
Districts	334	334	334
Observations	35,010	36,043	32,035

Notes.– * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. Each column presents a different estimation of equation 1. *Decentralized* is a dummy equaling 1 for districts with decentralized job centers and 0 otherwise. All continuous variables in logs. Regressions include a full set of dummies for districts and months. Standard errors given in parentheses are clustered at the job center and month level.

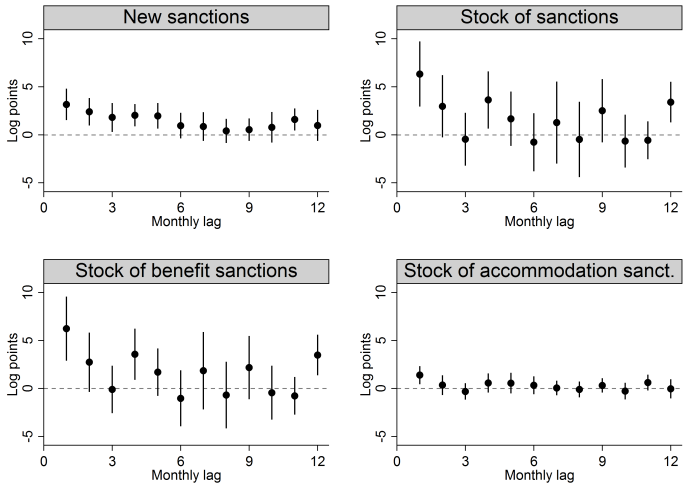
A.4 Correlation Analyses of ALMP and Sanction Effectiveness

Figure A.3: OLS: Correlations of Lagged Entries Into ALMP Measures With Job Finding



Notes.– The figures depict coefficients and their 95% confidence intervals from a simple regression of job finding on the lags of monthly inflow into job-creation schemes. The regression includes a full set of dummies for job centers and months. Standard errors are clustered at the job center and the month level.

Figure A.4: OLS: Correlations of Lagged Sanctions With Job Finding



Notes.– The figures depict coefficients and their 95% confidence intervals from a simple regression of job finding on the lags of monthly sanctions. The regression includes a full set of dummies for job centers and months. Standard errors are clustered at the job center and the month level.

A.5 ALMP Policies and Communal Elections

Decentralized job centers may use their autonomy strategically to reduce unemployment ahead of elections. Communal election dates vary by state and are usually held every 6 years. Hence, I normally observe two communal elections in each district, one before and one after the decentralization. This section provides a supplementary analysis where I augment the main model from equation (2.1) with an additional communal election variable. The election variable is hand-collected from the websites of the state's election administrations. I implement the election variable as a dummy equaling 1 in the month before an election and 0 otherwise. I add an interaction term of decentralization and elections to check whether decentralized job centers may use ALMPs more strategically ahead of elections than centralized providers.

Variable	(1) All ALMPs	(2) Short-term trainings	(3) Medium-term trainings	(4) Wage subsidies	(5) Job creation schemes
Decentralized	0.029	−0.071	−0.061	−0.047	0.293 ***
	(0.064)	(0.096)	(0.070)	(0.074)	(0.081)
Election	0.011	0.011	0.069	−0.024	0.012
	(0.033)	(0.030)	(0.063)	(0.041)	(0.063)
Decentralized	0.114 ***	0.074 ***	0.133	0.066	0.232 ***
×Election	(0.030)	(0.026)	(0.090)	(0.050)	(0.059)
R-squared	0.939	0.860	0.811	0.846	0.825
Districts	316	316	316	316	316
Observations	36656	36651	35027	36112	35290

Notes.– * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. Each column presents a different estimation of equation 2.1. The dependent variables are inflows of unemployed into the respective ALMP categories. *Decentralized* is a dummy equaling 1 for districts with decentralized job centers and 0 otherwise. Election variable is a dummy equaling 1 in the month before a communal election and 0 otherwise. Regressions include the stocks and flows of unemployed and vacancies as well as a full set of dummies for job centers and months. All continuous variables in logs. Sample sizes vary due to missing observations. Standard errors given in parentheses are clustered at the job center and the month level.

Table A.4: Communal Elections and ALMP Policies Under Decentralization

Table A.4 presents the results following the structure of Table 2.6. The first row presents the effect of decentralization on ALMP policies independent of any elections and confirms the previous results. In the next row of coefficients, I check whether elections have a general effect on ALMP policies. The estimated coefficients indicate that communal elections in general do not alter inflows into ALMP programs. However, communal elections after decentralization are associates with higher total ALMP inflows, as given by the next row of coefficients. Aside from

short-term trainings, the overall increase particularly stems from higher assignments into job creation schemes. This is suggestive evidence that decentralized providers may use their autonomy to reduce local unemployment ahead of communal elections.

A.6 Further Sensitivity Analyses

A.6.1 Conditional Difference-in-Differences

If observable characteristics influence the unobserved labor market trends of centralized and decentralized job centers, reweighing our observations with regard to these characteristics should reinforce the validity of the common trends assumption. Therefore, we employ a variant of the conditional difference-in-differences estimator (see Heckman et al., 1997, 1998). This estimator balances the treatment and the control observations with regard to their fundamental characteristics before running the difference-in-differences regression. Usually, balancing is performed on the propensity score which requires estimating potentially restrictive probit or logit models in the first place. In contrast, we use entropy balancing which is a non-parametric method. Entropy balancing assigns each control unit a non-negative weight such that the reweighted control group and the treatment group match exactly in terms of pre-specified sample moments of their covariate distributions (Hainmueller, 2012).

We balance the growth rates of major population and labor market groups. Table A.5 presents these mean growth rates and the statistical significance of their differences across subsamples before and after matching. It turns out that the mean growth rates were quite similar already before applying entropy balancing. Yet, entropy balancing further reduces any differences.

	Treated	Unbalanced Control		Balanced Control	
Variable	Mean	Mean	P-Value	Mean	P-Value
GDP per capita	22.970	24.099	0.578	22.970	1.000
Civil labor force	−4.053	−3.337	0.517	−4.052	1.000
Young (15–24 yr)	−2.231	0.245	0.382	−2.231	1.000
Prime-aged (25–54 yr)	−4.611	−4.361	0.808	−4.610	0.999
Old (55–64 yr)	−2.702	−1.763	0.530	−2.702	1.000
Foreign nationals	1.079	6.711	0.017 **	1.088	0.997
Employment	1.125	2.862	0.138	1.127	0.999
Agriculture	−14.563	−13.323	0.696	−14.561	1.000
Mining and energy	−2.042	1.540	0.490	−2.040	1.000
Manufacturing	−7.174	−6.951	0.931	−7.172	1.000
Construction	−15.049	−16.063	0.667	−15.052	0.999
Trade, transp., comm.	0.974	3.123	0.217	0.975	0.999
Finance and real estate	21.660	25.296	0.170	21.663	0.999
Public and priv. services	9.043	9.737	0.669	9.043	1.000
Job-center unemployment	−11.523	−13.497	0.309	−11.525	0.999
Young (15–24 yr)	−14.847	−19.351	0.282	−14.851	0.999
Prime-aged (25–54 yr)	−13.299	−15.246	0.312	−13.302	0.999
Old (55–64 yr)	7.777	9.107	0.708	7.779	1.000
Foreign nationals	−10.500	−12.917	0.301	−10.501	1.000
Population on welfare	−7.451	−8.826	0.322	−7.453	0.999
Young (15–24 yr)	−13.762	−15.478	0.421	−13.765	0.999
Prime-aged (25–54 yr)	−10.174	−11.706	0.290	−10.176	1.000
Old (50–64 yr)	5.320	4.634	0.559	5.319	1.000
Foreign nationals	−4.323	−7.023	0.119	−4.325	0.999
	40	290		290	

Notes.– P-values given for t-test of mean equality. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table A.5: Balancing of mean growth rates for 2000–2010 (GDP, population, employment) or 2007–2010 (unemployment, welfare)

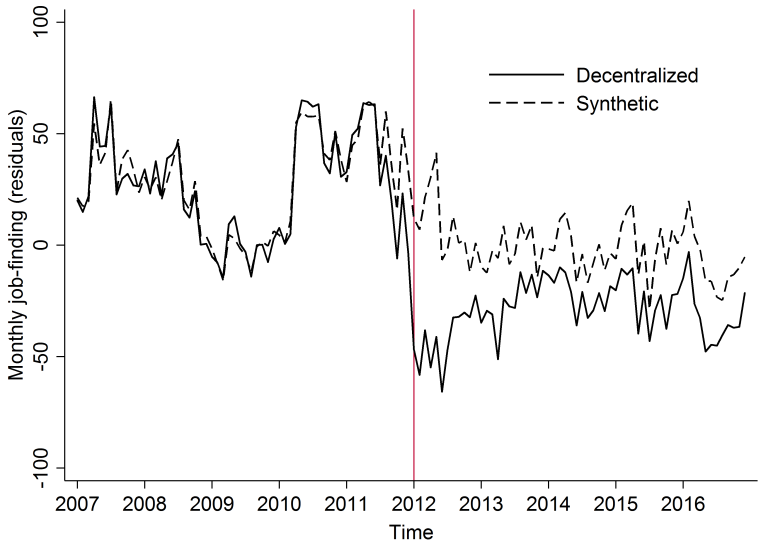
A.6.2 Synthetic Control

We implement the synthetic control method by constructing a synthetic counterfactual as a linear combination of the control group districts for each treated district. The resulting synthetic control unit is then used to extrapolate the counterfactual evolution of job finding of the treated unit for the post-treatment period.

The linear combination is chosen such that the synthetic control unit resembles the treated unit's job-finding flow during the first half of the pre-intervention period as closely as possible. We use the second half of the pre-treatment interval as a validation period to confirm the model's validity. 'Closeness' is measured as the Mean Squared Prediction Error (MSPE). Predictions are based on observed stocks and inflows of unemployed and vacancies, as well as the shares of young, old, and foreign individuals among the total stock of unemployed. All data are demeaned and seasonally adjusted.

Figure A.5 presents the resulting evolution of the average job-finding flow of treated and synthetic control units. Across the entire pre-treatment period, the job-finding flows in both groups are almost identical, suggesting the synthetic control group successfully replicates the evolution of decentralized districts. With the decentralization in 2012, job finding in affected districts declined significantly relative to the synthetic control observations. After about one year, the job finding in treated districts slowly converges to the synthetic control group again but stabilizes at a lower level. On average, job finding in decentralized districts is around 10% below synthetic levels, consistent with our DiD estimates.

Figure A.5: Synthetic Control Approach

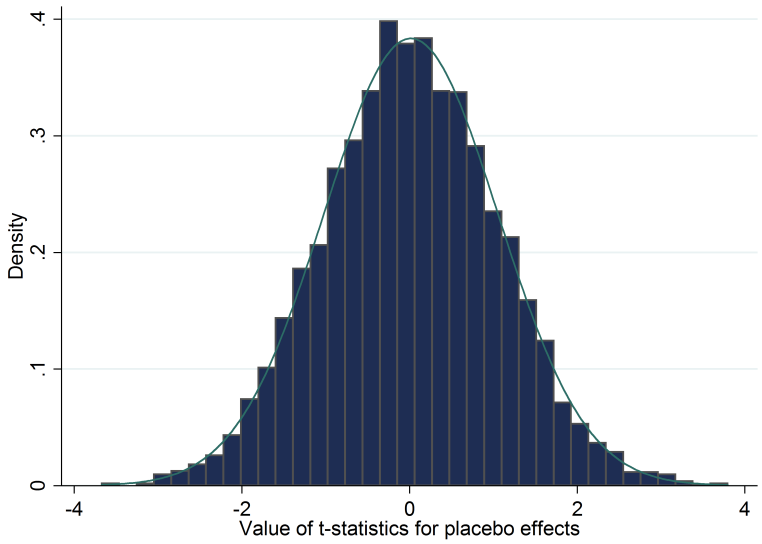


Notes.– Time-labels (x-axis) refer to January of a given year. Synthetic control approach with seasonally adjusted job finding as the outcome variable, i.e residuals from a regression of monthly job finding levels per district on eleven month dummies and an intercept. Donor pools for synthetic control units include all districts not decentralizing in 2012. Predictor variables include all covariates from the baseline regression as well as the shares of young, old, and foreign individuals among the total stock of unemployed. The second half of the pre-treatment interval is used as a validation period. Synthetic control was computed for each treated district individually and then averaged across all decentralizing districts.

A.6.3 Empirical Monte-Carlo Simulation

Figure A.6 refers to an empirical Monte-Carlo simulation following Huber et al. (2013), where randomly chosen non-reforming districts receive placebo treatments to confirm that our standard errors are correctly sized.

Figure A.6: Histogram of t-Statistics for Decentralization Coefficient from Placebo Treatments



Notes.– T-statistics computed from 5,000 estimations of equation (2.1) where *Decentralized* is a dummy equaling 1 for 41 randomly chosen districts with centralized job centers and 0 otherwise. Regressions include the stocks and flows of unemployed and vacancies as well as a full set of dummies for job centers and months. Standard errors are two-way clustered on the job center and the month level.

A.6.4 Sensitivity Analyses for ALMP Inflows and Sanction Flows

(subsequent pages)

Variable	(1) Base specification	(2) Denied applicants as only controls	(3) Denied applicants as treated	(4) Over-subscription subsample	(5) Conditional DiD	(6) Controls without non-treated neighbors	(7) Spatial lag in X	(8) X measured at commuting zone level
Decentralized	0.033	−0.037	0.089 **	0.037	0.016	0.026	0.013	−0.021
	(0.064)	(0.073)	(0.039)	(0.065)	(0.055)	(0.065)	(0.063)	(0.067)
R-squared	0.939	0.910	0.944	0.931	0.921	0.932	0.940	0.936
Districts	316	75	276	292	312	206	316	316
Observations	36972	8775	32292	34164	36504	24102	36972	36972

Notes.– * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. Each column presents a different estimation of equation (2.1). *Decentralized* is a dummy equaling 1 for districts with decentralized job centers and 0 otherwise. Standard errors given in parentheses are clustered at the job center and the month level. Standard errors for column 5 were obtained by 200 bootstrap replications of the combined balancing and estimation procedure. Regressions include the stocks and flows of unemployed and vacancies as well as a full set of dummies for job centers and months.

Table A.6: Difference-in-Differences: Effect of Decentralization on Log Monthly Total Inflows Into All ALMP measures Under Different Specifications

Variable	(1) Base specification	(2) Denied applicants as only controls	(3) Denied applicants as treated	(4) Over-subscription subsample	(5) Conditional DiD	(6) Controls without non-treated neighbors	(7) Spatial lag in X	(8) X measured at commuting zone level
Decentralized	0.299 ***	0.091	0.209 **	0.300 ***	0.249 ***	0.285 ***	0.301 ***	0.242 ***
	(0.080)	(0.115)	(0.095)	(0.083)	(0.090)	(0.084)	(0.081)	(0.081)
R-squared	0.825	0.798	0.825	0.815	0.817	0.823	0.825	0.822
Districts	316	75	276	292	312	206	316	316
Observations	35575	8570	31017	32771	35107	23302	35575	35575

Notes.– * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. Each column presents a different estimation of equation (2.1). *Decentralized* is a dummy equaling 1 for districts with decentralized job centers and 0 otherwise. Standard errors given in parentheses are clustered at the job center and the month level. Standard errors for column 5 were obtained by 200 bootstrap replications of the combined balancing and estimation procedure. Regressions include the stocks and flows of unemployed and vacancies as well as a full set of dummies for job centers and months.

Table A.7: Difference-in-Differences: Effect of Decentralization on Log Monthly Total Inflows Into Job Creation Schemes Under Different Specifications

Variable	(1) Base specification	(2) Denied applicants as only controls	(3) Denied applicants as treated	(4) Over-subscription subsample	(5) Conditional DiD	(6) Controls without non-treated neighbors	(7) Spatial lag in X	(8) X measured at commuting zone level
Decentralized	−0.063	−0.059	−0.016	−0.059	−0.090 *	−0.042	−0.059	−0.091
	(0.062)	(0.070)	(0.036)	(0.063)	(0.049)	(0.063)	(0.061)	(0.064)
R-squared	0.896	0.867	0.903	0.882	0.882	0.890	0.896	0.894
Districts	316	75	276	292	312	206	316	316
Observations	36804	8611	32286	33997	36336	23935	36804	36804

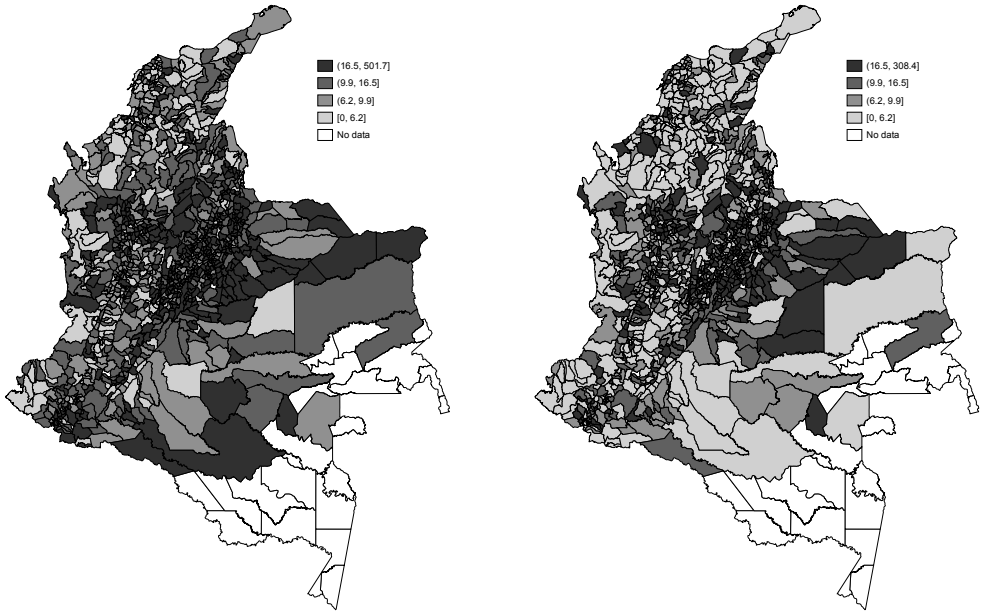
Notes.– * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. Each column presents a different estimation of equation (2.1). *Decentralized* is a dummy equaling 1 for districts with decentralized job centers and 0 otherwise. Standard errors given in parentheses are clustered at the job center and the month level. Standard errors for column 5 were obtained by 200 bootstrap replications of the combined balancing and estimation procedure. Regressions include the stocks and flows of unemployed and vacancies as well as a full set of dummies for job center and months.

Table A.8: Difference-in-Differences: Effect of Decentralization on Log Monthly Total New Sanctions Under Different Specifications

B Appendix to Chapter 3: Fueling Fiscal Interactions: Commodity Price Shocks and Local Government Spending in Colombia

B.1 Supplementary Figures and Tables

Figure B.1: Municipal Expenditures Per Capita on Sport and Recreation in Thousand 2008 Real COP by Quartiles of the Pooled Sample, 2000–2010



a. Year 2000 b. Year 2010

Table B.1: Variable Description

Variable	Description
Total	Total amount of money invested in the different categories of each municipality.
Health	Funds invested in health. Includes investment in new infrastructure, maintenance of existing infrastructure, promotion programs, as well as the salaries of the health centers' employees.
Education	Funds invested in education. Includes investment in new infrastructure, maintenance of existing infrastructure, promotion programs, as well as the salaries of teachers.
Water	Municipal investment in aqueducts, sewage systems, garbage collection, new sanitation infrastructure and maintenance of the existing one.
Housing	Accounts for subsidies to buy or improve houses for the poor and expenditures to improve housing conditions of the general population.
Sport and recreation	Funds used to promote sport and leisure activities including infrastructure, programs and instructors.
Agriculture	Payments made to improve the productivity of the agricultural sector like infrastructure, experimental farms and consultants.
Community development	Investment in programs to support public engagement and active citizenship.
Municipal equipment	Investment on municipal buildings, like public offices, market places, cemeteries, public places and slaughter houses.
Vulnerable groups	Investments used for programs directed towards vulnerable groups, including children, elderly, single mothers, displaced and disabled people.
Justice and security	Money used to pay the salaries of police officers and sheriffs, doctors, social workers, and psychologists working for the family service agency.
Disasters prevention	Investment used for disaster relief and prevention.

Source: Based on Acevedo and Bornacelly (2014).

Table B.2: Results for All Non-earmarked Categories

	W_y	Total excl. y	W_total excl. y	AP F-statistic excl. inst.
Housing	-2.091	0.498***	1.043	3.631
	(1.531)	(0.047)	(0.711)	
Agriculture	-2.690	0.371***	1.083	0.791
	(4.193)	(0.103)	(1.732)	
Com. development	1.757	0.144***	-0.324	2.824
	(1.650)	(0.030)	(0.305)	
Sport & rec.	1.257**	0.555***	-0.702**	13.99
	(0.576)	(0.030)	(0.303)	
Equipment	0.606	0.488***	-0.336	33.75
	(0.392)	(0.035)	(0.210)	
Vuln. groups	-2.600	0.353***	1.100	0.513
	(4.584)	(0.053)	(1.818)	
Justice	1.873*	0.309***	-0.496*	4.591
	(1.107)	(0.033)	(0.285)	
Dis. prevention	1.097	0.333***	-0.421	3.811
	(1.101)	(0.028)	(0.370)	

Notes: Each row represents a separate estimation for different non-earmarked spending categories. IV specification as in Table 3.3. Expenditures and transfers are real per capita values in 2008 COP. All continuous variables in logs. The Angrist-Pischke (AP) first-stage F-statistic of the excluded instrument is also reported. Standard errors given in parentheses are clustered on the municipality level. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table B.3: Spatial Lags as Instruments

	Health	Education	Water	Non-earmarked	Sport & rec.
W_y	0.130	0.372***	0.091	0.169	-0.100
	(0.136)	(0.073)	(0.071)	(0.124)	(0.075)
Population	5.036	17.598***	-5.832**	0.828	0.902
	(3.258)	(4.442)	(2.899)	(2.988)	(3.120)
Population squared	-2.287	-7.621***	2.618*	-0.713	-0.684
	(1.508)	(2.035)	(1.356)	(1.384)	(1.445)
Share of rural population	-0.362**	-1.171***	-0.021	-0.173	0.192
	(0.166)	(0.243)	(0.155)	(0.159)	(0.145)
Transfers	0.012***	0.020***	0.017***	0.028***	0.010***
	(0.003)	(0.003)	(0.003)	(0.004)	(0.003)
Total excl. y	0.265***	0.424***	0.592***	0.110***	0.531***
	(0.035)	(0.036)	(0.033)	(0.030)	(0.026)
Observations	12,023	12,023	12,023	12,023	12,023
Number of municipalies	1,093	1,093	1,093	1,093	1,093
Municipality FE	Yes	Yes	Yes	Yes	Yes
Department - year FE	Yes	Yes	Yes	Yes	Yes
AP F-statistic excl. inst.	46.46	198.4	189.2	42.95	168.3

Notes: In contrast to the main specification in Table 3.3, here, the spatial lags of all control variables are used as excluded instruments (traditional spatial IV estimator). Expenditures and transfers are real per capita values in 2008 COP. All continuous variables in logs. The Angrist-Pischke (AP) first-stage F-statistic of the excluded instrument is also reported. Standard errors given in parentheses are clustered on the municipality level. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table B.4: All Expenditure Categories as Separate Control Variables

	Total	Health	Education	Water	Non-earmarked	Sport & rec.
W_y	-0.081	2.114	0.304	0.282	0.550	1.391***
	(0.499)	(3.036)	(0.246)	(0.537)	(0.883)	(0.469)
Health			0.312***	0.318***		0.070***
			(0.019)	(0.020)		(0.014)
Education		0.326***		0.211***		0.089***
		(0.022)		(0.018)		(0.016)
Water		0.296***	0.181***			0.184***
		(0.029)	(0.015)			(0.014)
Sport & rec.		0.077***	0.079***	0.190***		
		(0.024)	(0.013)	(0.014)		
Housing		0.000	0.017***	0.032***		0.044***
		(0.012)	(0.005)	(0.006)		(0.007)
Agriculture		0.065***	0.071***	0.076***		0.109***
		(0.020)	(0.011)	(0.012)		(0.012)
Community development		-0.021*	0.018*	0.038***		0.031***
		(0.012)	(0.010)	(0.009)		(0.011)
Equipment		0.000	0.021***	0.035***		0.061***
		(0.010)	(0.006)	(0.006)		(0.007)
Vulnerable groups		0.007	0.008	0.075***		0.063***
		(0.017)	(0.008)	(0.009)		(0.010)
Justice		0.036	0.025**	0.074***		0.065***
		(0.039)	(0.010)	(0.010)		(0.012)
Disaster prevention		0.001	0.035***	0.031***		0.032***
		(0.012)	(0.008)	(0.008)		(0.010)
Observations	12,023	12,023	12,023	12,023	12,023	12,023
R-squared	0.045	-0.219	0.357	0.388	-0.041	-0.034
Number of municipalities	1,093	1,093	1,093	1,093	1,093	1,093
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes
Department - year FE	Yes	Yes	Yes	Yes	Yes	Yes
AP F-statistic excl. inst.	16.26	1.139	82.96	13.96	5.186	19.67

Notes: In contrast to the main specification in Table 3.3, here, for each expenditure category on the left hand side, all other spending categories and their respective spatial lags are included in the estimation as separate control variables. The respective spatial lags of the spending categories on the right hand side are also included in the estimations but are not displayed in the table for brevity. Expenditures and transfers are real per capita values in 2008 COP. All continuous variables in logs. The Angrist-Pischke (AP) first-stage F-statistic of the excluded instruments is also reported. Standard errors given in parentheses are clustered on the municipality level. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

B.2 Data Preparation and Imputation

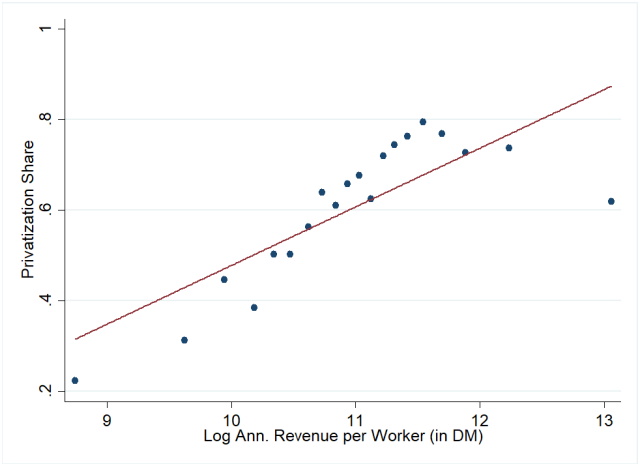
From the total number of 1122 annual municipality observations, we drop twenty units that maintain a special territorial status different from municipalities, being situated in very remote and sparsely populated areas. We also delete four new municipalities established after 2007, two remote islands and three municipalities left without direct neighbors. This leaves us with 1093 municipalities in our sample.

Missing values in expenditure variables account for less than 3% of the total municipality-year observations. For about 90% of the municipalities with missing values, we still observe at least nine years. Therefore, we decide against listwise deletion of municipalities with missing values and opt for imputation by linear intrapolation. For covariates, this procedure is necessary in less than 0.01% of all cases.

As a check of the data we compare the expenditure data with the *Ejecuciones Presupuestales Municipales* data base provided by the Colombian National Planning Department (DNP). While the overall consistency is very favorable, the comparison leads us to make some minor outlier corrections. In particular, we impute linear intrapolations for all values that are larger than twenty times a municipality's median in the same expenditure category and correct obvious decimal point errors. These changes affect approximatively another 3% of the observations but lead to an improvement of the data balance. Despite these needs for correction, the resulting dataset as described in Table 3.1 still has an exceptional quality given the developing country setting. One qualification remains: For the years 2006 and 2007, a total of 73 municipality-year pairs exhibit zero expenditures.

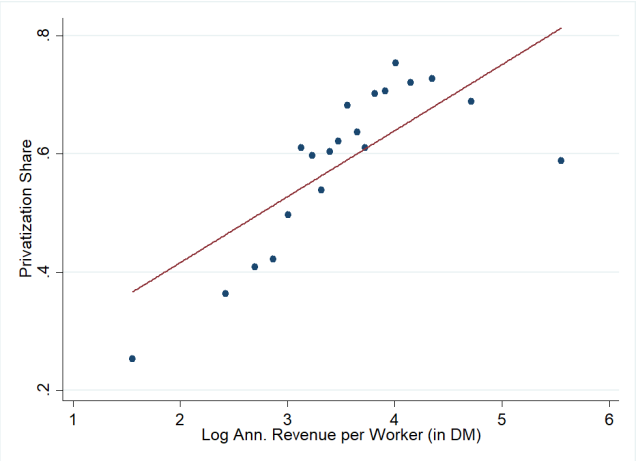
C Appendix to Chapter 4: Privatization Decisions and Economic Efficiency

C.1 Supplementary Figures



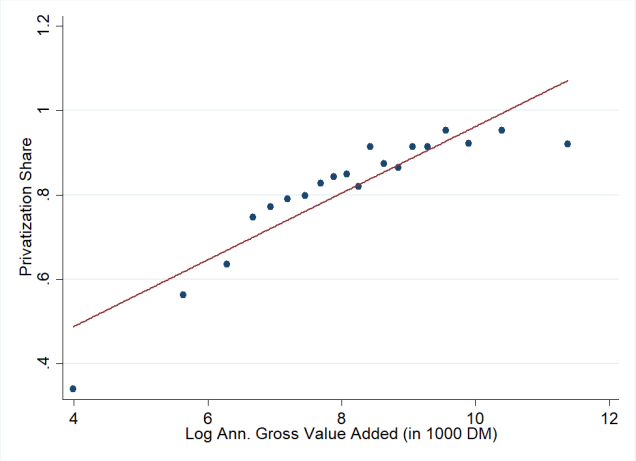
Notes: Dots represent binned scatter plot where the observations are grouped into 20 equally sized bins. A simple linear regression line indicates the direction of the correlation. Only uses observations from 1991 where the THA was still the owner.

Figure C.1: Privatization and Log Revenues per Worker, Treuhand-owned firms from 1991 only



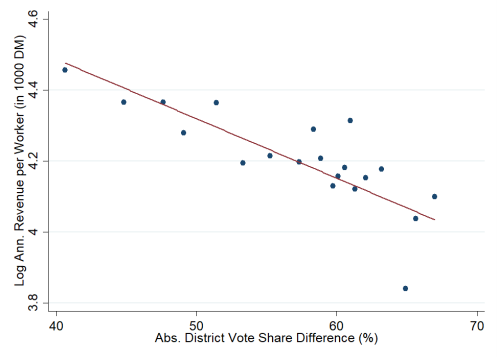
Notes: Dots represent binned scatter plot where the observations are grouped into 20 equally sized bins. A simple linear regression line indicates the direction of the correlation. Only uses observations from 1991 where the THA was still the owner.

Figure C.2: Privatization and Revenues per Hour, Treuhand-owned firms from 1991 only



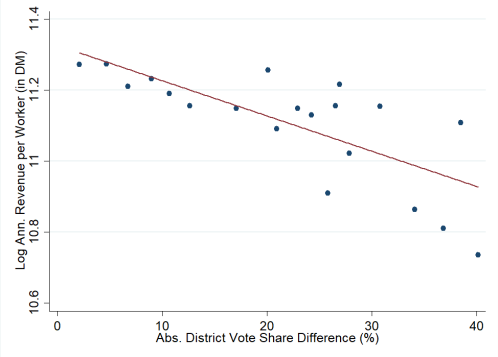
Note: Dots represent binned scatter plot where the observations are grouped into 20 equally sized bins. A simple linear regression line indicates the direction of the correlation.

Figure C.3: Privatization and Gross Value Added



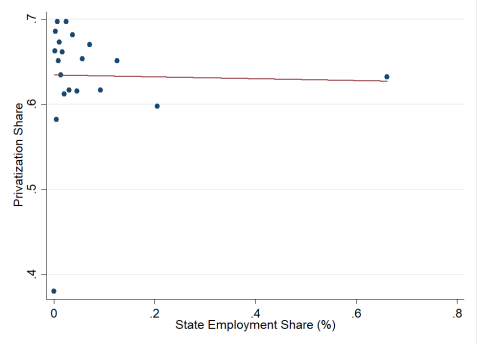
Note: Dots represent binned scatter plot where the observations are grouped into 20 equally sized bins. A simple linear regression line indicates the direction of the correlation.

Figure C.4: Productivity and Government Vote Shares



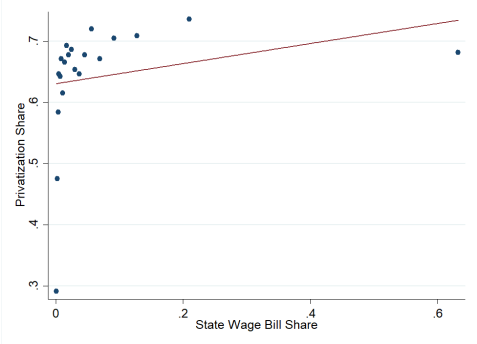
Note: Dots represent binned scatter plot where the observations are grouped into 20 equally sized bins. A simple linear regression line indicates the direction of the correlation.

Figure C.5: Productivity and Closeness of Election Results



Note: Dots represent binned scatter plot where the observations are grouped into 20 equally sized bins. A simple linear regression line indicates the direction of the correlation.

Figure C.6: Privatization and State Employment Shares without Winsorization



Note: Dots represent binned scatter plot where the observations are grouped into 20 equally sized bins. A simple linear regression line indicates the direction of the correlation.

Figure C.7: Privatization and State Wage Bill Shares without Winsorization

Note: Dots represent binned scatter plot where the observations are grouped into 20 equally sized bins. A simple linear regression line indicates the direction of the correlation.

Figure C.8: Productivity and Average Gross Wages

C.2 Supplementary Tables

Table C.1: Regression Results: Privatization and Alternative Productivity Indicators

	(1)	(2)	(3)
Revenue per Worker	0.068***		
	(0.012)		
Revenue per Hour		0.062***	
		(0.013)	
Gross Value Added			0.072***
			(0.005)
Industry FE	✓	✓	✓
State FE	✓	✓	✓
Survey FE	✓	✓	✓
R-squared	0.189	0.190	0.251
Observations	2,533	2,363	2,534

Notes.– * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. Each column presents a different estimation of equation (4.1). The outcome variable is a dummy equaling 1 if a firm was privatized at the end of 1994 and 0 otherwise. Productivity variables are in logs. Sample reduced to all firms with non-missing gross value added information. Robust standard errors given in parentheses.

Table C.2: Logit Results: Privatization and Alternative Productivity Indicators

	(1)	(2)	(3)
Revenue per Worker	0.465***		
	(0.083)		
Revenue per Hour		0.420***	
		(0.089)	
Gross Value Added			0.571***
			(0.046)
Industry FE	✓	✓	✓
State FE	✓	✓	✓
Survey FE	✓	✓	✓
R-squared			
Observations	2,327	2,172	2,327

Notes.– * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. Each column presents a different estimation of the logit equivalent of equation (4.1). The outcome variable is a dummy equaling 1 if a firm was privatized at the end of 1994 and 0 otherwise. Productivity variables are in logs. Sample reduced to all firms with non-missing gross value added information. Robust standard errors given in parentheses.

Table C.3: Regression Results: Privatization and Productivity Foundations

	(1)	(2)	(3)
Revenue per worker	0.107***	0.110***	0.107***
	(0.009)	(0.011)	(0.009)
Share of University Graduates		-0.001	
		(0.001)	
Share of R&D Workers			0.003***
			(0.001)
Industry FE	✓	✓	✓
State FE	✓	✓	✓
Survey FE	✓	✓	✓
R-squared	0.209	0.216	0.210
Observations	4,655	3,231	4,464

Notes.– * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. Each column presents a different estimation of equation (4.1). The outcome variable is a dummy equaling 1 if a firm was privatized at the end of 1994 and 0 otherwise. Productivity variables are in logs. Varying sample sizes due to item non-response. Robust standard errors given in parentheses.

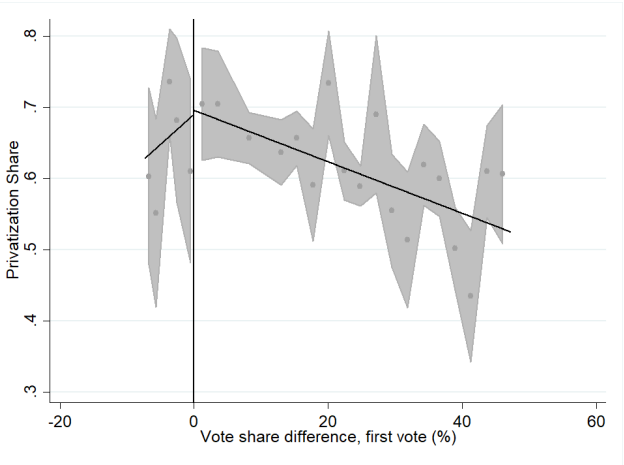
C.3 Privatization Decisions and Political Ideology: Exploiting Close Elections for a Potential Regression Discontinuity Design

Figure C.9 illustrates a potential regression discontinuity design to estimate the effect of party ideology of the incumbent constituency representative on local privatization. Close elections, where the succeeding candidate only won by a very narrow margin, have been frequently used to exploit quasi-experimental variation in the incumbent parties' ideologies (Lee and Lemieux, 2010). Hence, close elections could also provide a promising avenue to study political ideology as an additional non-economic factor in privatization decisions. However, East Germany consisted of only 61 electoral districts (excluding Berlin) at the federal election in 1990. The conservative party (CDU) won the majority of these districts by large margins. Thus, there is not a sufficient amount of district that experienced close elections where it is plausible that winning such an election is a random outcome. Plotting the given variation in election outcomes in Figure C.9 also does not indicate that a discontinuity in privatization decisions exists around the threshold where first and second-placed parties received similarly large vote shares.

C.4 The Treuhand Firm Surveys (Data Appendix)

This section provides an overview of the Treuhand survey dataset. It describes the collection, the coverage, and preparation for the entirety of all Treuhand surveys.

Figure C.9: Potential Regression Discontinuity Design



Note: Dots represent binned scatter plot where the observations are grouped into equally sized bins on each side of the cutoff. Separate linear regression lines on each side of the cutoff indicate the direction of the correlation.

It thereby goes beyond an incomplete series of reports which each summarize a single wave of Treuhand firm surveys (Kühl et al., 1991, 1992b,a; Wahse et al., 1993, 1996).

C.4.1 Data Collection

The first survey was based on an agreement between the Bundesanstalt für Arbeit and the Treuhand Agency.[1] As Treuhand companies accounted for about one third of East German employment, the survey was to act as an early warning system for the expected job losses across industries and locations (Kühl et al., 1991). The research institute of the Bundesanstalt, the IAB, commissioned the Berlin-based Soestra institute to conduct the survey.

The Soestra institute launched the first survey among Treuhand-owned companies in April 1991. Starting in October 1991, a related survey was conducted with companies already privatized by the Treuhand. Both surveys were repeated semi-annually until the Treuhand closed its operations in 1994. The survey of former Treuhand companies continued in annual intervals from 1995 until October 2003 (with the exception of October 2001). A total of 23 surveys was conducted.

For each wave, all firms owned by the THA at this time received a mail-sent questionnaire. Former THA firms received a questionnaire with mostly identical but also distinct items. Survey items varied across years. Formally, participation was not mandatory for firms. However, an official cover letter by the head of

[1] Leitlinien für eine Zusammenarbeit zwischen der Treuhandanstalt und der Bundesanstalt für Arbeit, 11 April 1991

Table C.4: Survey Characteristics by Date and Type of Survey

Survey	No. of Responses	No. of Variables
1991-Apr THA	5,261	30
1991-Oct Ex-THA	5,792	20
1991-Oct THA	5,792	27
1992-Apr Ex-THA	5,603	37
1992-Apr THA	5,603	42
1992-Oct Ex-THA	5,257	48
1992-Oct THA	5,257	56
1993-Apr Ex-THA	4,692	32
1993-Apr THA	4,692	35
1993-Oct Ex-THA	3,408	59
1993-Oct THA	3,408	61
1994-Apr Ex-THA	4,056	45
1994-Apr THA	4,056	45
1994-Oct Ex-THA	2,544	68
1994-Oct THA	2,544	70
1995-Oct Ex-THA	2,159	46
1996-Oct Ex-THA	818	25
1997-Oct Ex-THA	919	30
1998-Oct Ex-THA	965	14
1999-Oct Ex-THA	918	17
2000-Oct Ex-THA	824	17
2002-Oct Ex-THA	855	22
2003-Oct Ex-THA	750	16
N	44,821	

Notes: THA refers to surveys of currently Treuhand-owned companies. Ex-THA refers to survey directed to firms which left Treuhand ownership. For a few surveys, there are additional variables available which were beyond the scope of this data preparation.

the Treuhand accompanied the questionnaire, asking firms for a reply. Responses were to be sent back to the THA. Table C.4 describes the number of responses and variables covered by each survey wave. The number of responding Treuhand-owned firms is high in the early 90s but rapidly declines as all firms had to be privatized or liquidated by 1994. Inversely, the number of responses from former Treuhand companies increased as more and more firms left the public ownership.

C.4.2 Firms Covered

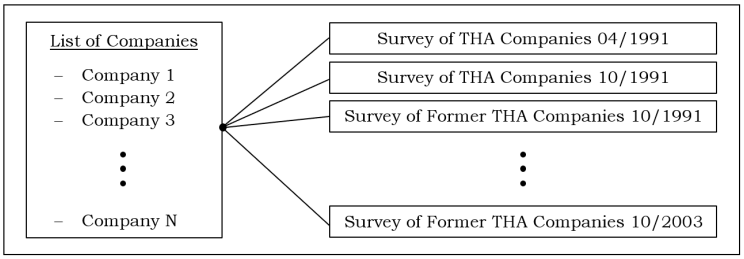
Questionnaires were sent out to all firms currently or formerly managed by the Treuhand between April 1991 and October 2003, except for privatized company parts. Plants with more than 100 employees were surveyed separately. Treuhand companies covered the entire industrial sector of the GDR as well as trading and large agricultural companies. These firms account for about one third of total East German employment. Not included were the former agricultural production cooperatives (LPGs), which went through a separate privatization process. Moreover, the survey left aside the public sector, including hospitals, schools, government administrations as well as non-civilian bodies.

C.4.3 Variables

The Treuhand questionnaires focus on the current and expected future employment structure but also include a broader range of topics. The following list gives an overview of the item categories repeatedly covered in the questionnaires.

- **Employment:** Overall current and expected future number of employees. In total, by gender, and by age categories.
  Overall number of employees can be categorized into the following groups (with intersections):
  - Apprentices: Number of current and expected future apprentices by gender.
  - Education and Qualification: Number of employees by degree of formal education/qualification obtained or required, e.g. untrained workers, trained specialists or university graduates.
  - Operating Fields: Current and expected future number of employees by gender and workplace, e.g. production, sales or controlling.
  - Vocational Training: Current and expected future number of employees participating in vocational training by gender, education/ qualification, training scheme and training objective as well as expenses for vocational training.

Figure C.10: Structure of the Treuhand Survey Dataset



  - New Hires: Number of employees that were newly hired by gender.
  - Job Vacancies: Number of job vacancies by function or department.
  - Short-time workers: Number of employees working short-time by gender and degree of working hours reduction.

- **Wages:** Wage expenses by gender.
- **Revenue:** Current and expected future revenue.
- **Investments:** Current and expected future gross investments, investment targets and investment impediments.
- **Legal status:** Legal status of the firm.
- **Treuhand department:** Department of the Treuhand that was in charge of the firm.
- **MBO, MBI:** Indicator whether firm was privatized through management buy-out or management buy-in.
- **Privatization date:** Year and month.
- **Industry:** The data differentiates 249 different industry sectors at the four-digit level.
- **Location:** Municipality, district and state of the firm's location.

Some survey items were only included in a single or few waves and therefore are not included in this overview but may provide additional insights into more specific research questions.

C.4.4 Raw Data Structure and Panel Creation

As illustrated in figure C.10, the original data consists of two data types: a list with basic background information of all THA companies and 23 separate cross-sectional survey data sets. The list of companies was provided by the THA itself

to the Soestra institute to conduct the surveys. It is based on the Treuhand's internal administrative data. For 14,961 companies, the list contains basic firm information, including industry code, location, privatization status, legal status, and the Treuhand department assigned to each firm. The fieldwork institution also used this list to record survey inclusion and participation. I drop the 1,385 firms from the list which were never considered for participation in any survey. An inspection reveals that this may has happened for good reason as their legal status is predominantly coded as "property title" ("Eigentumstitel"), implying that these firms are not independent and economically active companies.

The 23 cross-sectional survey datasets contain the firms' responses to the respective survey questionnaires. Recurring survey items may have different identifiers every time they were included in the surveys. Hence, I harmonize these variable identifiers manually across all surveys in order to obtain time-consistent variables.

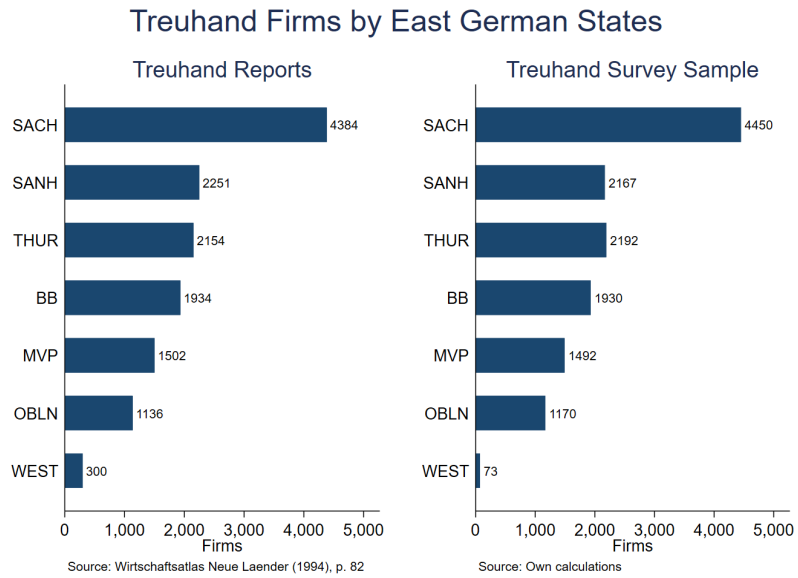
My matching procedure advances in two steps. Using unique Treuhand firm identifiers, I first append the cross-sectional surveys and merge them with the list of THA companies. Second, I use survey observations which did not match based on the Treuhand identifiers and match them based on their unique id from the Federal Insurance Fund for Salaried Employees (BfA). This results in an unbalanced panel of 13,576 firms where 10,167 companies (74.9%) match with at least one survey response. 3,409 firms from the list of companies do not have any matching responses, i.e. supposedly have never replied to a survey. On the other hand, 949 survey responses (2.1% of all responses) do not match with the list of companies. Plausible explanations for this finding are new firm owners, mergers, or surveys answered by a subsidiary company.

Several data cleaning procedures help to improve the data quality. I drop values outside of the plausible range in the district, region, legal status, and industry variable. As these variables are available from both sources, the list of companies and several surveys, I then combine these sources to fill missing values. I also exploit the hierarchy underlying the municipality, district, region, and state variables to impute missing higher-level geographic indications where lower-level information is available.

C.4.5 Data Assessment

The analysis of a new panel survey requires a careful assessment of the overall data quality. There are two major concerns. First, the list of companies used to conduct the survey may not be representative of the entire population of Treuhand firms. If representativeness is in doubt, any survey conducted using this list will be in doubt too. Second, even if the companies initially approached to take part in the study are representative, non-random response will threaten the surveys' validity. Both concerns require further analysis presented in the following sections.

Figure C.11: Comparison of Treuhand Data and Official Statistics by State



Representativeness of the Company List

The list of companies includes basic firm variables with a very low degree of missing values. Investigating the representativeness of the company list is generally difficult as the Treuhand's original firm database ISUD ("Informationssystem Unternehmensdatenbank") has not been made available to research and may suffer from digital obsolescence as the necessary soft- and hardware providing access is no longer available.[2] However, it is possible to compare the list of companies with aggregated official statistics published by the THA based on the ISUD. In this section, I provide such a comparison by East German states, industries, and firm status when the THA ceased operating in 1994.

Drawing upon official Treuhand data, the Gebhardt (1994) publishes the number of THA companies by German states. According to this source, Saxony (SACH) was home to 4,384 companies, the largest count among all East German states. On the other hand, (East) Berlin only accounted for 1,136 companies. Figure C.11 shows the comparison with the respective geographical distribution of firms in the company list (right-hand side). The figure reveals that the data from the company list closely matches the official statistics for all East German states, which is a first confirmation of the data's representativeness.

The Bundesanstalt für vereinigungsbedingte Sonderaufgaben (1994) offers official statistics for the distribution of firms by 3-digit industry and their status in December 1994. Figure C.12 summarizes the official number of firms by industry to the company list data. The three most frequent industries, mechanical engineering,

[2]This concern refers to a corresponding oral statement by a research fellow of the Institut für Zeitgeschichte München-Berlin, who is involved in the historical investigation of the THA archives.

Figure C.12: Comparison of Treuhand Data and Official Statistics by Industry

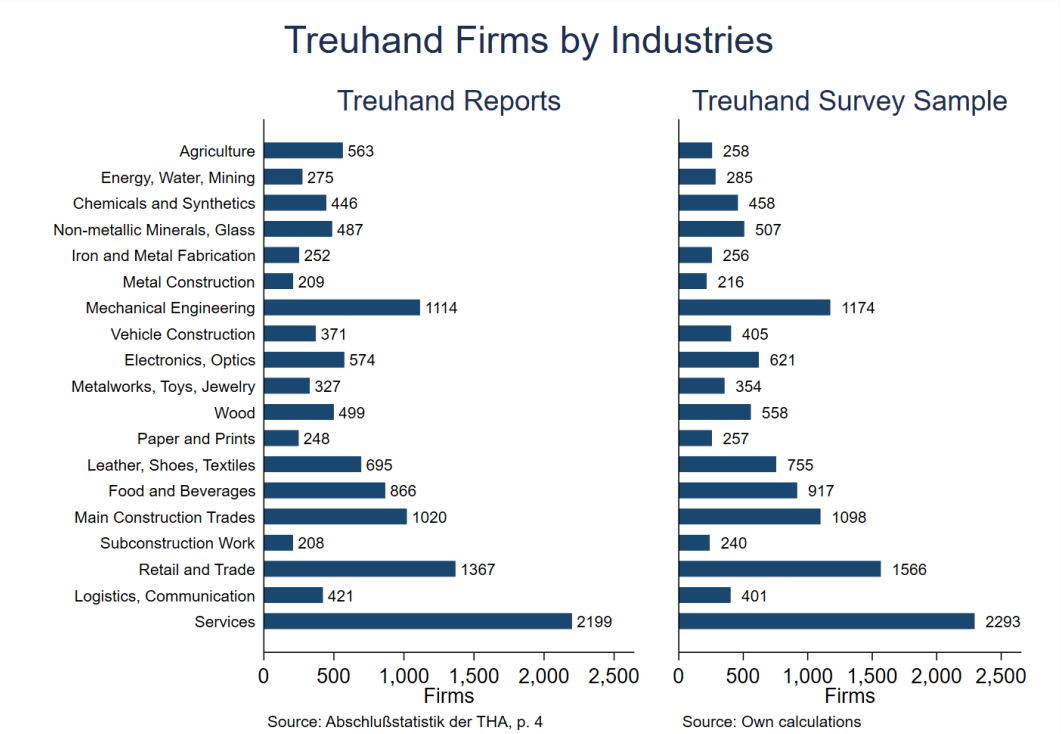
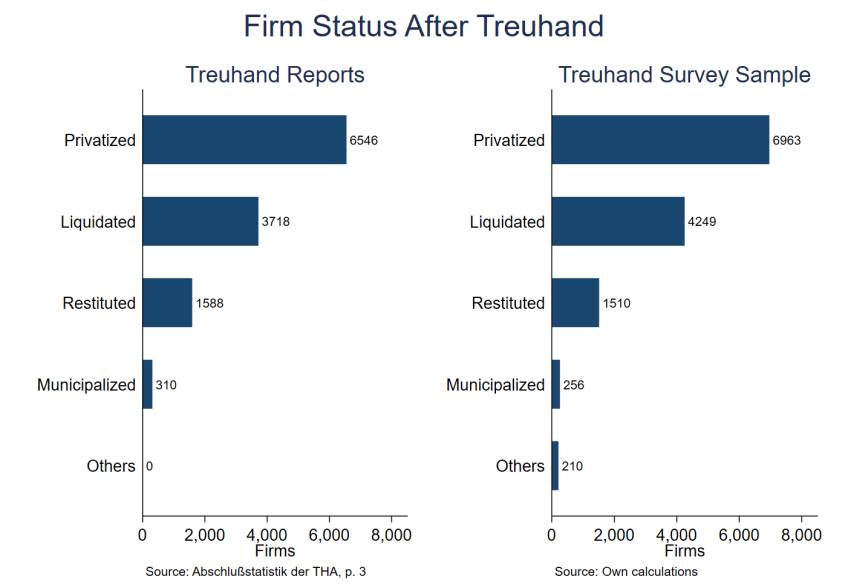


Figure C.13: Comparison of Treuhand Data and Official Statistics by Firm Status



trade, and services, are all represented in very similar numbers in both sources. The same holds true for all smaller industries except for agriculture, an industry which constitutes a special case of privatization as laid out in section C.4.2. Regarding the firms' status in 1994, the Abschlußstatistik distinguishes among firms being privatized, liquidated, restituted to former owners, or municipalized. Figure C.13 shows the distribution of these outcomes in comparison to an analogously constructed variable based on the company list data. As can be seen, privatization is by far the most common outcome with more than 6,500 cases in both sources. Liquidations, restitutions, and municipalizations occurred less often but at similar frequencies in both sources.

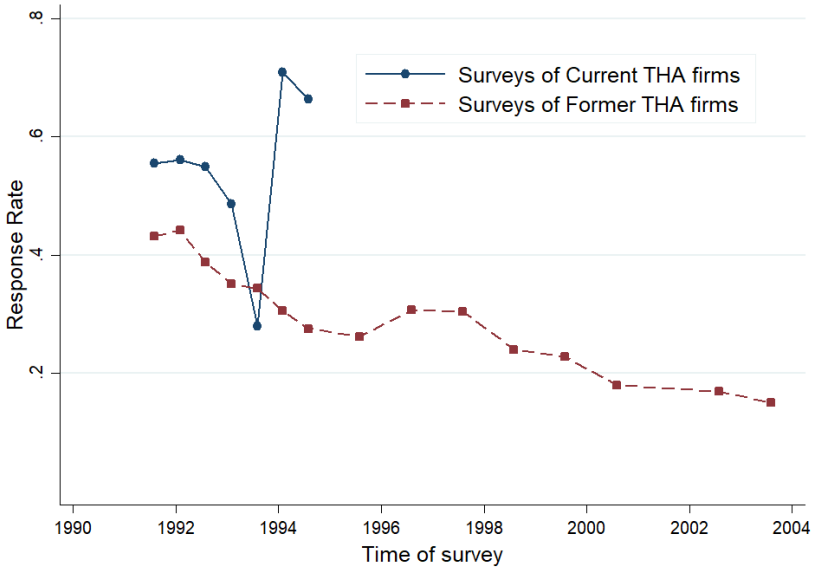
An exact match of the two sets of figures would be an unrealistic standard as partitions, mergers, and differing reporting dates impede direct comparisons. Despite of these challenges, the company list data closely resembles the numbers from original THA reports. Comparing data sources based on the distributions in three variables does of course not proof representativeness, yet it provides solid evidence for employing it as an acceptable working assumption. Whether representativeness can be assumed after taking survey response behavior into account will be explored in the next sections.

Rates of Survey Response

The second major concern regarding the external validity of the survey is non-random response. Firms may not participate in the survey for various reasons, such as insolvency, recent merger, lack of time, or privacy concerns. If these reasons are related to survey outcomes of interest, the resulting analysis will be biased. Ideally, the group of firms returning their questionnaires should mimic the population of all Treuhand companies. The Treuhand survey authors Kühl et al. (1991) assume structural equivalence of responding and non-responding firms due to the high response rate but do not provide an analysis to support their claim. I provide an overview of the overall number of responses across firms and surveys. In the next step, I study differences in the populations of surveyed and responding firms.

I define the response rate as the number of firms responding to the respective survey divided by the total number of firms surveyed. Figure C.14 presents the response rates by survey type. Overall, response rates were between 20 to 65 %. These figures are higher for the earlier waves of the survey and lower for later ones. Taking together all surveys from October 1991 to 2003, the total response rate amounts to 35.6%. Overall, the median firm is observed twice in the matched data set. 25% of the firms are responding in at least five surveys. Restricting the panel to the years 1991 to 2000, i.e. removing the least favorably responded survey years, the median number of firm observations rises to three. 25% of the 11,105 firms show up in at least six surveys.

Figure C.14: Response Rates by Survey Type



These response rates favorably compare to similar company surveys. A one-time firm survey of Treuhand companies by Dyck (AER 1997) in 1992 reached an effective response rate of 23%. The German KfW/ZEW Start-up Panel realized a response rate of 26% (Fryges et al., 2009), the Survey on the Access to Finance of Enterprises (SAFE) by the European Central Bank achieved 14%, and the IAB's Establishment Panel reaches rates of up to 40% for first-time respondents (Janik and Kohaut, 2012). However, the response rate itself is only a weak indicator of distortion from firms' sorting into survey responses. To detect non-random sorting, it is necessary to study the deviation of firm characteristics between the resulting sample and the original population. This is the topic of the following section.

Non-response Bias

A non-response analysis is feasible as the list of companies plausibly provides a quasi-census for Treuhand firms. It records which firms were included in the surveys and offers a basic set of auxiliary variables available for all companies. The first Treuhand survey from April 1991 is the only exception where firms' inclusion was not recorded. Linking the list with the cross-sectional surveys thus allows for a direct comparison of deviations in firm characteristics between respondents and the population of firms included for all surveys starting from October 1991. To study non-response bias, I adopt the convention by Sakshaug and Huber (2016) and define the Non-Respone Bias$_{i,s}$ for characteristic i and survey s as follows

$$\text{Non-Response Bias}_{i,s} = \bar{Y}_{i,s} - \bar{Y}^*_{i,s}$$

where $\bar{Y}_{i,s} - \bar{Y}^*_{i,s}$ is the difference between the share of responding firms and the share of all firms included in the respective survey which satisfy the respective characteristic. Below, I present firm characteristics and non-response biases for the surveys of currently Treuhand-owned companies and subsequently for the surveys of former Treuhand companies.

Table C.5 presents characteristics of the firms participating in the surveys of current Treuhand firms. Each column refers to a separate survey, allowing a comparison within surveys and over time. The typical firm in the survey of October 1991, for example, is a corporation in the mechanical engineering or vehicles industry from a rural area in Saxony which will be privatized or liquidated at equal probabilities (46%) by the end of 1994. Given that firms endogenously leave Treuhand ownership and thereby fall out from this survey type, firm characteristics are remarkably stable over time. The typical firm in the October 1994 survey is still a corporation in the mechanical engineering or vehicles industry yet from a rural district in Saxony-Anhalt.

The respective non-response biases for the same variables and surveys are summarized in Table C.6. For instance, firms responding to the April 1991 survey are one percentage point (pp) more frequently registered as a corporations. Across all surveys and variables, non-response biases below five percentage points are the norm. The only remarkable exception is the survey from October 1993 whose respondents are nine percentage points more likely to be in industries developing more favorably between 1991 and 1994 than the population of firms included in the survey. Similarly, these firms also are more often privatized and less often privatized at the end of the year 1994. While non-response biases exhibit low magnitudes overall, over-representation of economically more promising firms is a concern for this particular survey wave. Therefore, at a minimum, any analysis based on these surveys should provide a robustness check omitting this wave.

Table C.7 shows firm characteristics for respondents of the former THA company surveys'. For instance, the typical responding firm in the October 1991 survey is a privatized corporation in the building industry from a large city in Saxony. As more firms leave THA ownership, the respondents' sample composition changes over time. The building industry, for example, accounts for 18% of all respondents in October 1991 but only for 8% 12 years later in 2003. Conversely, the share of firms from energy, mechanical engineering, logistics, and services modestly increases over the same period.

A systematic non-response analysis for former THA companies yields favorable results as Table C.8 reveals. The non-response bias measures never exceed a difference of five percentage points across all variables for the initial nine surveys. A larger difference emerges only in 1997: Responses from large cities are 8 percentage points less frequent compared to the total firm population surveyed with similar

Table C.5: Characteristics of Responding Firms in Surveys of Current Treuhand Companies (Shares)

	(1) THA91H	(2) THA92F	(3) THA92H	(4) THA93F	(5) THA93H	(6) THA94F	(7) THA94H
Legal Status							
Corporation	0.99	0.98	0.98	0.98	0.97	0.98	0.99
Other Legal Status	0.01	0.02	0.02	0.02	0.03	0.02	0.01
Industry							
Agriculture	0.01	0.01	0.01	0.01	0.00	0.01	0.01
Energy, Water, Mining	0.02	0.02	0.02	0.02	0.02	0.02	0.05
Chemicals, Synthetics	0.05	0.05	0.05	0.05	0.05	0.04	0.06
Wood, Ceramics, Glass	0.09	0.08	0.09	0.08	0.07	0.06	0.03
Metal	0.08	0.08	0.08	0.09	0.12	0.07	0.18
Mech. Engineering, Vehicles	0.17	0.18	0.17	0.17	0.22	0.17	0.20
Electronics, Optics	0.06	0.06	0.06	0.06	0.04	0.07	0.04
Textiles, Paper, Prints	0.12	0.13	0.14	0.15	0.13	0.15	0.05
Food	0.08	0.07	0.06	0.06	0.05	0.06	0.01
Building, Construction	0.06	0.06	0.04	0.03	0.05	0.02	0.01
Logistics, Trade	0.14	0.15	0.16	0.16	0.10	0.18	0.06
Services	0.12	0.11	0.12	0.13	0.16	0.15	0.30
Rel. empl. change 91-94	0.45	0.44	0.44	0.46	0.57	0.48	0.60
Post-Privatization Status							
Privatized	0.46	0.43	0.31	0.24	0.44	0.13	0.59
Management buy-out	0.05	0.05	0.03	0.01	0.02	0.00	0.00
Management buy-in	0.10	0.08	0.04	0.02	0.03	0.01	0.00
Liquidated	0.46	0.49	0.63	0.71	0.47	0.84	0.16
Restituted	0.05	0.05	0.04	0.03	0.03	0.00	0.00
Municipalized	0.01	0.01	0.01	0.00	0.00	0.00	0.00
State							
Brandenburg	0.15	0.14	0.13	0.14	0.17	0.16	0.19
Mecklenburg-West Pom.	0.10	0.10	0.10	0.10	0.10	0.10	0.10
Berlin	0.09	0.08	0.08	0.09	0.06	0.10	0.11
Saxony	0.34	0.34	0.34	0.36	0.28	0.34	0.21
Saxony-Anhalt	0.17	0.17	0.16	0.15	0.22	0.15	0.25
Thuringia	0.15	0.16	0.18	0.17	0.17	0.15	0.13
District							
Large cities	0.29	0.29	0.30	0.31	0.24	0.31	0.30
Urban areas	0.14	0.14	0.14	0.14	0.15	0.13	0.12
Rural areas w urban hubs	0.27	0.27	0.26	0.28	0.29	0.27	0.36
Rural areas	0.30	0.31	0.29	0.28	0.33	0.29	0.22

Notes.– Shares of all responding firms satisfying the respective characteristic given. *Rel. Empl. Incr. 91-94* denotes the share of firms from industries with an above-median employment development between October 1991 and October 1994. *Post-Privatization Status* refers to a company's ownership situation after the Treuhand ceased to exist at the turn of the year 1994/1995. The *district* classification stems from the German Federal Institute for Research on Building, Urban Affairs and Spatial Development (BBSR).

tendencies in the following waves. However, such differences do not appear for other variables. A particular concern is again the sorting of economically more

Table C.6: Non-Response Bias in Surveys of Current Treuhand Companies (pp/100)

	(1) THA91H	(2) THA92F	(3) THA92H	(4) THA93F	(5) THA93H	(6) THA94F	(7) THA94H
Legal Status							
Corporation	0.01	0.01	0.02	0.03	0.01	0.02	0.03
Other Legal Status	-0.01	-0.01	-0.02	-0.03	-0.01	-0.02	-0.03
Industry							
Agriculture	-0.00	-0.00	-0.00	0.00	-0.00	0.00	0.00
Energy, Water, Mining	0.00	0.00	0.00	0.01	0.00	0.00	0.01
Chemicals, Synthetics	0.01	0.00	0.00	0.00	0.00	-0.00	0.01
Wood, Ceramics, Glass	-0.00	-0.01	0.00	-0.01	-0.01	-0.01	-0.01
Metal	0.00	0.00	0.00	0.00	0.03	-0.01	0.03
Mech. Engineering, Vehicles	0.03	0.03	0.02	0.03	0.06	0.01	0.01
Electronics, Optics	0.01	-0.00	-0.00	-0.00	-0.03	0.00	-0.01
Textiles, Paper, Prints	0.02	0.01	0.01	0.02	-0.01	0.02	-0.02
Food	0.00	-0.00	-0.01	-0.01	-0.02	-0.00	-0.00
Building, Construction	-0.02	-0.01	-0.01	-0.01	0.01	-0.01	-0.00
Logistics, Trade	-0.01	-0.01	0.01	-0.00	-0.06	-0.00	-0.06
Services	-0.04	-0.03	-0.02	-0.02	0.02	-0.00	0.04
Rel. empl. incr. 91-94	-0.04	-0.02	-0.02	-0.01	0.09	-0.01	0.05
Post-Privatization Status							
Privatized	-0.03	0.02	0.02	0.04	0.22	-0.04	-0.03
Management buy-out	0.01	0.01	0.00	0.00	0.01	-0.00	-0.00
Management buy-in	-0.01	0.01	-0.00	0.01	0.02	0.00	-0.01
Liquidated	0.05	-0.02	-0.01	-0.04	-0.27	0.04	0.00
Restituted	-0.01	-0.01	-0.01	-0.00	0.01	-0.00	-0.01
Municipalized	-0.00	-0.00	-0.00	-0.00	0.00	-0.00	-0.00
State							
Brandenburg	0.01	0.00	0.00	0.01	0.03	0.01	0.02
Mecklenburg-West Pom.	-0.01	-0.00	0.00	-0.00	0.01	0.00	-0.01
Berlin	-0.01	-0.00	0.00	-0.00	-0.02	-0.00	-0.01
Saxony	0.01	0.01	-0.00	0.01	-0.06	-0.01	-0.03
Saxony-Anhalt	0.00	0.00	-0.00	-0.02	0.04	-0.01	0.02
Thuringia	-0.01	-0.01	0.00	0.00	0.00	0.01	0.01
District							
Large cities	-0.02	-0.02	-0.01	-0.01	-0.06	-0.01	-0.02
Urban areas	0.01	0.01	0.01	0.00	0.01	-0.01	-0.00
Rural areas w urban hubs	0.01	0.00	-0.00	0.01	0.02	0.01	0.03
Rural areas	0.01	0.01	0.00	-0.00	0.03	0.00	-0.00

Notes.– Shares of all responding firms satisfying the respective characteristic given. *Rel. Empl. Incr. 91-94* denotes the share of firms from industries with an above-median employment development between October 1991 and October 1994. *Post-Privatization Status* refers to a company's ownership situation after the Treuhand ceased to exist at the turn of the year 1994/1995. The *district* classification stems from the German Federal Institute for Research on Building, Urban Affairs and Spatial Development (BBSR).

viable firms into the survey. The indicator most closely related to economic performance, the share of firms from industries developing favorably between 1991 and 1994, demonstrates that the difference between respondents and the survey

Table C.7: Characteristics of Responding Firms in Surveys of Former Treuhand Companies (Shares)

	(1) Ex-THA91H	(2) Ex-THA92F	(3) Ex-THA92H	(4) Ex-THA93F	(5) Ex-THA93H	(6) Ex-THA94F	(7) Ex-THA94H	(8) Ex-THA95H	(9) Ex-THA96H	(10) Ex-THA97H	(11) Ex-THA98H	(12) Ex-THA99H	(13) Ex-THA00H	(14) Ex-THA02H	(15) Ex-THA03H
Legal Status															
Corporation	0.97	0.95	0.95	0.93	0.93	0.93	0.92	0.97	0.95	0.95	0.92	0.92	0.93	0.92	0.92
Other Legal Status	0.03	0.05	0.05	0.07	0.07	0.07	0.08	0.03	0.05	0.05	0.08	0.08	0.07	0.08	0.08
Industry															
Agriculture	0.01	0.01	0.01	0.01	0.02	0.01	0.01	0.01	0.01	0.02	0.01	0.01	0.01	0.01	0.02
Energy, Water, Mining	0.01	0.01	0.02	0.02	0.03	0.03	0.04	0.07	0.06	0.05	0.04	0.04	0.05	0.05	0.06
Chemicals, Synthetics	0.04	0.04	0.05	0.05	0.04	0.05	0.05	0.05	0.06	0.07	0.06	0.06	0.05	0.06	0.05
Wood, Ceramics, Glass	0.09	0.09	0.10	0.09	0.09	0.09	0.08	0.07	0.08	0.07	0.09	0.09	0.08	0.08	0.08
Metal	0.07	0.07	0.08	0.08	0.07	0.07	0.08	0.08	0.07	0.09	0.07	0.07	0.08	0.08	0.08
Mech. Engineering, Vehicles	0.12	0.14	0.13	0.15	0.14	0.15	0.15	0.18	0.17	0.15	0.17	0.17	0.15	0.16	0.14
Electronics, Optics	0.06	0.05	0.04	0.05	0.05	0.05	0.05	0.06	0.04	0.05	0.06	0.06	0.04	0.05	0.04
Textiles, Paper, Prints	0.07	0.05	0.05	0.05	0.06	0.06	0.06	0.04	0.06	0.06	0.06	0.06	0.06	0.07	0.06
Food	0.10	0.09	0.09	0.08	0.08	0.07	0.06	0.07	0.07	0.07	0.07	0.07	0.08	0.08	0.08
Building, Construction	0.18	0.17	0.16	0.15	0.15	0.13	0.14	0.11	0.12	0.10	0.11	0.11	0.11	0.10	0.08
Logistics, Trade	0.09	0.10	0.10	0.12	0.11	0.10	0.11	0.09	0.10	0.10	0.11	0.11	0.13	0.11	0.12
Services	0.15	0.19	0.17	0.16	0.16	0.17	0.18	0.18	0.17	0.17	0.15	0.15	0.16	0.15	0.18
Rel. empl. change 91-94	0.57	0.57	0.57	0.54	0.53	0.53	0.54	0.49	0.51	0.53	0.50	0.50	0.52	0.49	0.53
Post-Privatization Status															
Privatized	0.73	0.74	0.75	0.74	0.73	0.73	0.73	0.95	0.76	0.74	0.70	0.70	0.70	0.71	0.70
Management buy-out	0.05	0.05	0.06	0.06	0.06	0.06	0.05	0.07	0.04	0.04	0.05	0.05	0.04	0.03	0.04
Management buy-in	0.17	0.22	0.21	0.19	0.19	0.17	0.15	0.20	0.19	0.17	0.16	0.16	0.16	0.15	0.13
Liquidated	0.09	0.07	0.06	0.05	0.04	0.05	0.02	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Restituted	0.14	0.15	0.16	0.15	0.17	0.16	0.17	0.00	0.15	0.15	0.21	0.21	0.21	0.21	0.20
Municipalized	0.03	0.03	0.03	0.03	0.04	0.03	0.04	0.00	0.06	0.06	0.07	0.07	0.08	0.07	0.08
State															
Brandenburg	0.11	0.14	0.15	0.19	0.17	0.15	0.14	0.15	0.14	0.16	0.15	0.15	0.14	0.13	0.15
Mecklenburg-West Pom.	0.13	0.13	0.12	0.11	0.13	0.10	0.11	0.11	0.13	0.12	0.12	0.12	0.11	0.11	0.12
Berlin	0.08	0.07	0.07	0.06	0.05	0.08	0.07	0.07	0.05	0.05	0.04	0.04	0.04	0.02	0.03
Saxony	0.35	0.35	0.33	0.32	0.31	0.35	0.33	0.31	0.33	0.32	0.33	0.33	0.33	0.36	0.32
Saxony-Anhalt	0.13	0.14	0.17	0.15	0.19	0.17	0.18	0.18	0.17	0.18	0.17	0.17	0.18	0.17	0.17
Thuringia	0.19	0.16	0.16	0.16	0.14	0.16	0.18	0.19	0.19	0.18	0.18	0.18	0.19	0.20	0.21
District															
Large cities	0.32	0.31	0.31	0.28	0.26	0.29	0.26	0.27	0.24	0.22	0.21	0.21	0.22	0.20	0.20
Urban areas	0.11	0.12	0.12	0.13	0.13	0.14	0.13	0.11	0.14	0.14	0.14	0.14	0.15	0.15	0.14
Rural areas w urban hubs	0.30	0.27	0.27	0.28	0.28	0.26	0.29	0.31	0.32	0.33	0.31	0.31	0.30	0.33	0.31
Rural areas	0.27	0.30	0.30	0.31	0.33	0.31	0.31	0.31	0.30	0.31	0.33	0.33	0.33	0.33	0.35

Notes.– Shares of all responding firms satisfying the respective characteristic given. *Rel. Empl. Incr. 91-94* denotes the share of firms from industries with an above-median employment development between October 1991 and October 1994. *Post-Privatization Status* refers to a company's ownership situation after the Treuhand ceased to exist at the turn of the year 1994/1995. The *district* classification stems from the German Federal Institute for Research on Building, Urban Affairs and Spatial Development (BBSR).

Table C.8: Non-Response Bias in Surveys of Former Treuhand Companies (pp/100)

	(1) Ex-THA91H	(2) Ex-THA92F	(3) Ex-THA92H	(4) Ex-THA93F	(5) Ex-THA93H	(6) Ex-THA94F	(7) Ex-THA94H	(8) Ex-THA95H	(9) Ex-THA96H	(10) Ex-THA97H	(11) Ex-THA98H	(12) Ex-THA99H	(13) Ex-THA00H	(14) Ex-THA02H	(15) Ex-THA03H
Legal Status															
Corporation	0.01	0.04	0.03	0.02	0.02	0.02	0.02	-0.00	0.00	0.01	-0.00	-0.00	0.03	-0.01	0.01
Other Legal Status	-0.01	-0.04	-0.03	-0.02	-0.02	-0.02	-0.02	0.00	-0.00	-0.01	0.00	0.00	-0.03	0.01	-0.01
Industry															
Agriculture	0.00	-0.01	-0.00	-0.00	-0.00	-0.00	-0.00	-0.00	-0.00	0.01	-0.01	-0.01	0.00	0.00	0.00
Energy, Water, Mining	-0.00	0.00	0.00	0.01	0.00	0.01	0.01	0.03	0.03	0.02	0.01	0.01	0.02	0.02	0.03
Chemicals, Synthetics	0.01	0.01	0.01	0.01	0.01	0.01	0.01	-0.00	0.01	0.02	0.02	0.02	0.02	0.02	0.01
Wood, Ceramics, Glass	-0.00	-0.00	0.00	0.00	0.00	0.00	-0.01	-0.01	-0.01	-0.01	0.01	0.01	-0.01	-0.00	-0.01
Metal	0.01	0.01	0.01	0.01	0.01	0.01	0.01	0.01	-0.02	0.01	-0.00	-0.00	0.01	0.01	0.01
Mech. Engineering, Vehicles	-0.00	0.02	0.01	0.02	0.02	0.02	0.01	0.01	0.00	0.01	0.02	0.02	0.02	0.01	0.01
Electronics, Optics	0.01	0.00	0.00	0.00	0.01	0.01	0.01	0.01	-0.01	-0.00	0.01	0.01	-0.00	0.01	0.00
Textiles, Paper, Prints	0.01	-0.00	-0.00	-0.00	0.00	0.00	-0.00	-0.01	-0.01	-0.00	-0.01	-0.01	0.00	0.01	-0.01
Food	-0.01	-0.00	0.01	-0.00	0.00	-0.00	-0.01	0.00	-0.00	0.01	-0.00	-0.00	-0.00	0.01	0.01
Building, Construction	0.03	0.01	0.01	0.01	0.01	-0.01	0.00	-0.03	-0.01	-0.01	-0.02	-0.02	-0.02	-0.03	-0.05
Logistics, Trade	-0.03	-0.02	-0.03	-0.02	-0.03	-0.03	-0.02	-0.00	0.00	-0.04	-0.00	-0.00	-0.00	-0.03	-0.01
Services	-0.02	-0.01	-0.03	-0.03	-0.03	-0.02	-0.01	0.01	0.01	-0.00	-0.02	-0.02	-0.03	-0.03	-0.01
Rel. empl. incr. 91-94	0.01	-0.01	0.01	-0.01	-0.01	-0.01	-0.00	-0.02	-0.00	0.03	-0.02	-0.02	-0.02	-0.02	-0.01
Post-Privatization Status															
Privatized	0.02	0.01	0.02	0.02	0.01	0.00	-0.01	0.02	-0.02	0.02	-0.03	-0.03	-0.04	-0.03	-0.05
Management buy-out	0.00	0.00	0.00	0.01	0.01	0.01	0.00	-0.00	-0.02	-0.01	-0.01	-0.01	0.00	-0.02	-0.00
Management buy-in	0.02	0.03	0.03	0.02	0.03	0.01	-0.00	0.01	0.01	0.03	-0.00	-0.00	0.01	-0.01	-0.03
Liquidated	-0.03	-0.02	-0.02	-0.02	-0.02	-0.00	-0.03	-0.02	-0.01	-0.06	-0.01	-0.01	-0.00	-0.03	-0.00
Restituted	0.02	0.01	0.00	-0.01	0.01	-0.01	-0.00	0.00	0.00	0.02	0.01	0.01	-0.01	0.04	0.00
Municipalized	-0.01	0.00	-0.00	0.01	0.01	0.00	0.01	0.00	0.03	0.03	0.03	0.03	0.04	0.04	0.04
State															
Brandenburg	-0.02	-0.02	-0.01	0.04	0.02	0.00	-0.01	0.00	-0.01	0.01	0.01	0.01	0.00	-0.02	0.01
Mecklenburg-West Pom.	0.01	0.00	0.00	-0.00	0.02	-0.01	-0.00	-0.00	0.02	0.01	0.01	0.01	-0.00	0.00	0.01
Berlin	0.01	-0.01	-0.01	-0.02	-0.02	0.01	-0.01	-0.02	-0.01	-0.03	-0.02	-0.02	-0.02	-0.05	-0.03
Saxony	0.00	0.03	0.01	-0.00	-0.03	0.01	-0.00	-0.01	0.00	-0.00	-0.00	-0.00	-0.01	0.03	-0.03
Saxony-Anhalt	0.00	0.00	0.01	-0.00	0.04	0.01	0.01	0.01	-0.01	0.02	-0.00	-0.00	0.03	0.01	0.00
Thuringia	-0.00	-0.00	-0.00	-0.00	-0.02	-0.00	0.01	0.02	0.01	0.00	0.01	0.01	0.01	0.03	0.04
District															
Large cities	-0.01	0.01	-0.00	-0.03	-0.04	-0.00	-0.03	-0.05	-0.04	-0.08	-0.04	-0.04	-0.06	-0.09	-0.08
Urban areas	0.00	0.01	-0.00	0.00	-0.01	0.00	-0.00	-0.00	0.00	0.01	0.00	0.00	0.01	0.02	-0.00
Rural areas w urban hubs	0.02	-0.00	0.01	0.01	0.02	-0.00	0.02	0.02	0.03	0.06	0.02	0.02	0.02	0.05	0.03
Rural areas	-0.01	-0.01	0.00	0.02	0.03	0.00	0.01	0.03	0.01	0.01	0.02	0.02	0.03	0.02	0.05

Notes.– Shares of all responding firms satisfying the respective characteristic given. *Rel. Empl. Incr. 91-94* denotes the share of firms from industries with an above-median employment development between October 1991 and October 1994. *Post-Privatization Status* refers to a company's ownership situation after the Treuhand ceased to exist at the turn of the year 1994/1995. The *district* classification stems from the German Federal Institute for Research on Building, Urban Affairs and Spatial Development (BBSR).

population never exceeds three percentage points. Hence, the analysis supports the assumption that non-response bias is not a major concern for the data of former THA companies.

Combining the results from the analysis of both survey types, I conclude that the survey responses provide a useful picture of the pool of firms included in the survey. Four caveats remain. First, the argument holds for the variables under examination but may not do so for factors unobserved in this study. Second, the absence of major non-response bias within each survey does not imply representativeness across surveys. Survey results from 1996 may not be representative for the population of the initial Treuhand firms in 1990. Instead, representativeness is only given for the subset of surviving companies. Third, given the study excluded privatized company parts, the survey results only represent companies that were privatized as a whole. Fourth, lacking records prohibit a non-response analysis for April 1991. However, due to the particularly high number of responses available, representativeness is also plausible for this wave.

I close the data quality analysis with a few reflections on item non-response. The availability of basic firm information is favorable. Industry information is given for 95% of all firms, districts for 89%, legal status for 89%, the end of the Treuhand administration for 87%, and the federal employment agency for 69% of all cases. Disregarding the surveys after 2000, these values increase to 99%, 91%, 89%, 88%, and 91% respectively.

C.4.6 Particularities

There are several particularities that researchers working with the data need to be aware of.

- Several items in the surveys of 2002 and 2003, such as revenues, investments, and profits refer to the year prior to the survey. The same survey items in previous surveys refer to the respective current year.
- Important items such as revenues and investments are only included in the questionnaires starting in April 1992.
- Financial variables are not adjusted for inflation but are given in values of the current year.

C.4.7 Conclusion

The Treuhand survey data provides a major step towards analyzing the Treuhand privatizations, one of the world's most ambitious privatization programs. This step is significant as no major firm-level data source has been available covering these events. Being a major advantage of the Treuhand surveys, companies were

surveyed from 1991 to 2003 – that is almost from the start of the privatization efforts. Due to the survey origin of the data, I assess the data quality in more detail and generate two major insights. First, the list of company underlying the survey collection corresponds to the full population of THA companies. Second, exploiting auxiliary firm information originally provided by the THA, I find that the responses to individual surveys are hardly harmed by non-random response problems. Hence, the data enables studies on a wide range of interesting topics to gain a deeper understanding of privatizations processes and German economic reunification.

Bibliography

Abadie, Alberto, Alexis Diamond, and Jens Hainmueller (2010). Synthetic Control Methods for Comparative Case Studies: Estimating the Effect of California's Tobacco Control Program. *Journal of the American Statistical Association 105*(490), 493–505.

Abadie, Alberto, and Javier Gardeazabal (2003). The Economic Costs of Conflict: A Case Study of the Basque Country. *American Economic Review 93*(1), 113–132.

Abbring, Jaap H., Gerard J. van den Berg, and Jan C. van Ours (2005). The Effect of Unemployment Insurance Sanctions on the Transition Rate from Unemployment to Employment. *The Economic Journal 115*(505), 602–630.

Acemoglu, Daron, Amy Finkelstein, and Matthew J Notowidigdo (2013). Income and Health Spending: Evidence from Oil Price Shocks. *Review of Economics and Statistics 95*(4), 1079–1095.

Acemoglu, Daron, Suresh Naidu, Pascual Restrepo, and James A Robinson (2014, March). Democracy Does Cause Growth. NBER Working Paper 20004, National Bureau of Economic Research, forthcoming in the Journal of Political Economy.

Acemoglu, Daron, James A. Robinson, and Rafael J. Santos (2013, January). The Monopoly of Violence: Evidence from Colombia. *Journal of the European Economic Association 11*, 5–44.

Acevedo, Karina, and Iván Bornacelly (2014). Panel Municipal del CEDE. Serie Documentos CEDE 26, Universidad de los Andes.

Acosta, Olga Lucia, and Richard M Bird (2005). The Dilemma of Decentralization in Colombia. *Fiscal Reform in Colombia, The MIT Press, Cambridge-London*.

Agostini, Claudio A, Philip Brown, and Xiaobo Zhang (2016). Neighbour Effects in the Provision of Public Goods in a Young Democracy: Evidence from China. *Pacific Economic Review 21*(1), 13–31.

Agrawal, David R (2015). The Tax Gradient: Spatial Aspects of Fiscal Competition. *American Economic Journal: Economic Policy 7*(2), 1–29.

Agrawal, David R (2016). Local Fiscal Competition: An Application to Sales Taxation with Multiple Federations. *Journal of Urban Economics 91*, 122–138.

Ahlin, Åsa, and Eva Mörk (2008). Effects of Decentralization on School Resources. *Economics of Education Review 27*(3), 276–284.

Akerlof, George A., Andrew K. Rose, Janet L. Yellen, and Helga Hessenius (1991). East Germany in from the Cold: The Economic Aftermath of Currency Union. *Brookings Papers on Economic Activity 22*(1), 1–106.

Akin, John, Paul Hutchinson, and Koleman Strumpf (2005). Decentralisation and Government Provision of Public Goods: The Public Health Sector in Uganda. *The Journal of Development Studies 41*(8), 1417–1443.

Alesina, Alberto, Alberto Carrasquilla, and Juan Jose Echevarria (2005). Decentralization in Colombia. In A. Alesina (Ed.), *Institutional Reforms: The Case of Colombia*, pp. 175–208. Cambridge, MA: MIT Press.

Allers, Maarten A., and J. Paul Elhorst (2005, August). Tax Mimicking and Yardstick Competition Among Local Governments in the Netherlands. *International Tax and Public Finance 12*(4), 493–513.

Andrews, Martyn J., Steve Bradley, Dave Scott, and Richard Upward (2013). Estimating the Stock-Flow Matching Model Using Micro Data. *Journal of the European Economic Association 11*(5), 1153–1177.

Anselin, Luc (2008). A Companion to Theoretical Econometrics. In B. H. Baltagi (Ed.), *A Companion to Theoretical Econometrics*. Wiley.

Anselin, Luc (2010). Thirty Years of Spatial Econometrics. *Papers in Regional Science 89*(1), 3–25.

Arni, Patrick, Rafael Lalive, and Jan C. van Ours (2013). How Effective Are Unemployment Benefit Sanctions? Looking Beyond Unemployment Exit. *Journal of Applied Econometrics 28*(7), 1153–1178.

Arze del Granado, Javier, Jorge Martinez-Vazquez, and R. Simatupang (2008). Local Government Fiscal Competition in Developing Countries: The Case of Indonesia. *Urban Public Economics Review 1*(8), 13–45.

Baicker, Katherine (2005, February). The Spillover Effects of State Spending. *Journal of Public Economics 89*(2-3), 529–544.

Baicker, Katherine, Jeffrey Clemens, and Monica Singhal (2012). The Rise of the States: U.S. Fiscal Decentralization in the Postwar Period. *Journal of Public Economics 96*(11), 1079 – 1091. Fiscal Federalism.

Baicker, Katherine, and Nora Gordon (2006). The Effect of State Education Finance Reform on Total Local Resources. *Journal of Public Economics 90*(8), 1519–1535.

Banzhaf, H. Spencer, and B. Andrew Chupp (2012). Fiscal Federalism and Interjurisdictional Externalities: New Results and an Application to US Air Pollution. *Journal of Public Economics 96*(5-6), 449 – 464.

Barankay, Iwan, and Ben Lockwood (2007). Decentralization and the Productive Efficiency of Government: Evidence from Swiss Cantons. *Journal of Public Economics 91*(5-6), 1197–1218.

Bardhan, Pranab (2002). Decentralization of Governance and Development. *Journal of Economic Perspectives 16*(4), 185–205.

Baskaran, Thushyanthan (2012). Soft Budget Constraints and Strategic Interactions in Subnational Borrowing: Evidence from the German States, 1975–2005. *Journal of Urban Economics 71*(1), 114–127.

Baskaran, Thushyanthan (2014). Identifying Local Tax Mimicking with Administrative Borders and a Policy Reform. *Journal of Public Economics 118*, 41–51.

Behaghel, Luc, Bruno Crépon, and Marc Gurgand (2014). Private and Public Provision of Counseling to Job Seekers: Evidence from a Large Controlled Experiment. *American Economic Journal: Applied Economics 6*(4), 142–174.

Behncke, Stefanie, Markus Frölich, and Michael Lechner (2010). A Caseworker like Me – Does the Similarity Between the Unemployed and their Caseworkers Increase Job Placements? *The Economic Journal 120*(549), 1430–1459.

Bel, Germà, and Xavier Fageda (2007). Why Do Local Governments Privatise Public Services? A Survey of Empirical Studies. *Local Government Studies 33*(4), 517–534.

Bennmarker, Helge, Erik Grönqvist, and Björn Öckert (2013). Effects of Contracting out Employment Services: Evidence from a Randomized Experiment. *Journal of Public Economics 98*, 68–84.

Bertrand, Marianne, Esther Duflo, and Sendhil Mullainathan (2004). How Much Should We Trust Differences-in-Differences Estimates? *The Quarterly Journal of Economics 119*(1), 249–275.

Besley, T, and A Case (1995). Incumbent Behavior: Vote-Seeking, Tax-Setting, and Yardstick Competition. *The American Economic Review 38*(1), 25–45.

Besley, Timothy, and Stephen Coate (2003). Centralized versus Decentralized Provision of Local Public Goods: A Political Economy Approach. *Journal of Public Economics 87*(12), 2611 – 2637.

Biais, Bruno, and Enrico Perotti (2002, feb). Machiavellian Privatization. *American Economic Review 92*(1), 240–258.

Bird, Richard, and Michael Smart (2002). Intergovernmental Fiscal Transfers: International Lessons for Developing Countries. *World Development 30*(6), 899–912.

Bird, Richard M (2012). Fiscal Decentralization in Colombia: A Work (still) in Progress. International Center for Public Policy Working Paper Series 79, Andrew Young School of Policy Studies, Georgia State University.

Black, Dan A., Jeffrey A. Smith, Mark C. Berger, and Brett J. Noel (2003). Is the Threat of Reemployment Services More Effective than the Services Themselves? Evidence from Random Assignment in the UI System. *The American Economic Review 93*(4), 1313–1327.

Blanchard, Olivier, and Andrei Shleifer (2001). Federalism With and Without Political Centralization: China versus Russia. *IMF Economic Review 48*(1), 171–179.

Blundell, Richard, Monica Costa Dias, Costas Meghir, and John Reenen (2004). Evaluating the Employment Impact of a Mandatory Job Search Program. *Journal of the European economic association 2*(4), 569–606.

Bollinger, Christopher R, and Amitabh Chandra (2005). Iatrogenic Specification Error: A Cautionary Tale of Cleaning Data. *Journal of Labor Economics 23*(2), 235–257.

Bonet, Jaime, Gerson Perez, and Jhordland Ayala (2014). Contexto histórico y evolución del SGP en Colombia.

Boockmann, Bernhard, Stephan L. Thomsen, Thomas Walter, Christian Göbel, and Martin Huber (2015). Should Welfare Administration be Centralized or Decentralized? Evidence from a Policy Experiment. *German Economic Review 16*(1), 13–42.

Boone, Jan, Abdolkarim Sadrieh, and Jan C. van Ours (2009). Experiments on Unemployment Benefit Sanctions and Job Search Behavior. *53*(8), 937 – 951.

Borck, Rainald, Marco Caliendo, and Viktor Steiner (2007). Fiscal Competition and the Composition of Public Spending: Theory and Evidence. *FinanzArchiv: Public Finance Analysis 63*(2), 264–277.

Boycko, Maxim, Andrei Shleifer, and Robert W Vishny (1996). A Theory of Privatisation. *The Economic Journal*, 309–319.

Brollo, Fernanda, Katja Kaufmann, and Eliana La Ferrara (2015). The Political Economy of Enforcing Conditional Welfare Programs: Evidence from Brazil. Technical report, mimeo.

Brown, J. David, John S. Earle, and Álmos Telegdy (2006, feb). The Productivity Effects of Privatization: Longitudinal Estimates from Hungary, Romania, Russia, and Ukraine. *Journal of Political Economy 114*(1), 61–99.

Brown, J. David, John S. Earle, and Álmos Telegdy (2010, aug). Employment and Wage Effects of Privatisation: Evidence from Hungary, Romania, Russia and Ukraine. *The Economic Journal 120*(545), 683–708.

Bundesagentur für Arbeit (2012). Verbleib nach Austritt aus arbeitsmarktpolitischen Instrumenten - im Rechtskreis SGB II. Technical report, Bundesagentur für Arbeit.

Bundesanstalt für vereinigungsbedingte Sonderaufgaben (1994, 12). Abschlußstatistik der Treuhandanstalt per 31.12.1994. Brochure.

Bundesanstalt für Vereinigungsbedingte Sonderaufgaben (2003). *"Schnell privatisieren, entschlossen sanieren, behutsam stilllegen": ein Rückblick auf 13 Jahre Arbeit der Treuhandanstalt und der Bundesanstalt für Vereinigungsbedingte Sonderaufgaben*. Berlin: Wegweiser Verlag.

Bundeswahlleiter (2016). *Endgültiges Ergebnis nach Wahlkreisen der Bundestagswahl 1990*. Statistisches Bundesamt.

Burchardi, Konrad B., and Tarek A. Hassan (2013). The Economic Impact of Social Ties: Evidence from German Reunification. *The Quarterly Journal of Economics 128*(3), 1219–1271.

Caldeira, Emilie, Martial Foucault, and Gregoire Rota-Graziosi (2014). Decentralization in Africa and the Nature of Local Governments' Competition: Evidence from Benin. *International Tax and Public Finance 22*(6), 1048–1076.

Card, David, Jochen Kluve, and Andrea Weber (2010). Active Labour Market Policy Evaluations: A Meta-Analysis. *Economic Journal 120*(548), F452–F477.

Card, David, Jochen Kluve, and Andrea Weber (2017). What Works? A Meta Analysis of Recent Active Labor Market Program Evaluations. *Journal of the European Economic Association 16*(3), 894–931.

Carlin, Wendy, and Colin Mayer (1994). The Treuhandanstalt: Privatization by state and market. In O. Blanchard, K. Froot, and J. Sachs (Eds.), *The Transition in Eastern Europe, Volume 2: Restructuring*, pp. 189–214. University of Chicago Press.

Case, Anne C., Harvey S. Rosen, and James R. Hines (1993, October). Budget Spillovers and Fiscal Policy Interdependence. *Journal of Public Economics 52*(3), 285–307.

Chaparro, Juan Camilo, Michael Smart, and Juan Gonzalo Zapata (2005). Intergovernmental Transfers and Municipal Finance in Colombia. *Fiscal Reform in Colombia, the MIT Press, Cambridge-London*.

Christ, Peter, and Roger De Weck (1991). Das wäre ja schrecklich. *Die Zeit 1992*(22).

Coles, Melvyn G., and Eric Smith (1998). Marketplaces and Matching. *International Economic Review 39*(1), 239–254.

Crépon, Bruno, Esther Duflo, Marc Gurgand, Roland Rathelot, and Philippe Zamora (2013). Do Labor Market Policies have Displacement Effects? Evidence from a Clustered Randomized Experiment. *The Quarterly Journal of Economics 128*(2), 531–580.

Dalia Research (2018). Democracy Perception Index 2018. Technical report, Alliance of Democracies Foundation, Copenhagen.

De Fraja, Gianni, and Barbara Roberts (2009). Privatization in Poland. *The Economics of Transition 17*(3), 531–557.

Denisova, Irina, Markus Eller, Timothy Frye, and Ekaterina Zhuravskaya (2012, feb). Everyone Hates Privatization, But Why? Survey Evidence from 28 Post-Communist Countries. *Journal of Comparative Economics 40*(1), 44–61.

Deutscher Bundestag (2008). Bericht zur Evaluation der Experimentierklausel nach §6c des Zweiten Buches Sozialgesetzbuch. *Bundestagsdrucksache 16/11488*.

Diamond, Larry (2015). Facing up to the Democratic Recession. *Journal of Democracy 26*(1), 141–155.

Dinc, I Serdar, and Nandini Gupta (2011). The Decision to Privatize: Finance and Politics. *The Journal of Finance 66*(1), 241–269.

Dornbusch, Rudiger, Holger Wolf, and Lewis Alexander (1992). Economic Transition in Eastern Germany. *Brookings Papers on Economic Activity 1992*(1), 235–272.

Dornbusch, Rudiger, and Holger C. Wolf (1994, January). East German Economic Reconstruction. In *The Transition in Eastern Europe, Volume 1*, pp. 155–190. University of Chicago Press.

Doucouliagos, Hristos, and Mehmet Ali Ulubaşoğlu (2008). Democracy and Economic Growth: A Meta-Analysis. *American Journal of Political Science 52*(1), 61–83.

Drazen, Allan, and Marcela Eslava (2010, May). Electoral manipulation via voter-friendly spending: Theory and evidence. *Journal of Development Economics 92*(1), 39–52.

Dube, O., and J. F. Vargas (2013, February). Commodity Price Shocks and Civil Conflict: Evidence from Colombia. *The Review of Economic Studies 80*(4), 1384–1421.

Dustmann, Christian, Bernd Fitzenberger, Uta Schönberg, and Alexandra Spitz-Oener (2014). From Sick Man of Europe to Economic Superstar: Germany's Resurgent Economy. *Journal of Economic Perspectives 28*(1), 167–88.

Dyck, Alexander (1997). Privatization in Eastern Germany: Management Selection and Economic Transition. *American Economic Review 87*(4), 565–97.

Ebrahimy, Ehsan, and Robert Shimer (2010). Stock–Flow Matching. *Journal of Economic Theory 145*(4), 1325–1353.

Elhorst, J. Paul (2010, March). Applied Spatial Econometrics: Raising the Bar. *Spatial Economic Analysis 5*(1), 9–28.

Enikolopov, Ruben, and Ekaterina Zhuravskaya (2007). Decentralization and Political Institutions. *Journal of Public Economics 91*(11-12), 2261 – 2290.

Estrin, Saul, Jan Hanousek, Evžen Kočenda, and Jan Svejnar (2009). The Effects of Privatization and Ownership in Transition Economies. *Journal of Economic Literature 47*(3), 699–728.

Eugster, Beatrix, and Raphaël Parchet (2013). Culture and Taxes: Towards Identifying Tax Competition. Economics Working Paper Series 1339, University of St. Gallen, School of Economics and Political Science.

Faguet, Jean-Paul (2004). Does Decentralization Increase Government Responsiveness to Local Needs?: Evidence from Bolivia. *Journal of Public Economics 88*(3-4), 867–893.

Faguet, Jean Paul, and Fabio Sánchez (2013). Decentralization and Access to Social Services in Colombia. *Public Choice*, 1–23.

Fan, C Simon, Chen Lin, and Daniel Treisman (2009). Political Decentralization and Corruption: Evidence from around the World. *Journal of Public Economics 93*(1), 14–34.

Figlio, David, Van W. Kolpin, and William E. Reid (1999). Do States Play Welfare Games? *Journal of Urban Economics 46*(3), 437–454.

Fougère, Denis, Jacqueline Pradel, and Muriel Roger (2009). Does the Public Employment Service Affect Search Effort and Outcomes? *European Economic Review 53*(7), 818–833.

Freedom House (2018). Freedom in the World 2018. Democracy in crisis. *Freedom House*.

Frydman, R., C. Gray, M. Hessel, and A. Rapaczynski (1999, nov). When Does Privatization Work? The Impact of Private Ownership on Corporate Performance in the Transition Economies. *The Quarterly Journal of Economics 114*(4), 1153–1191.

Fryges, Helmut, Sandra Gottschalk, and Karsten Kohn (2009). The KfW/ZEW Start-up Panel: Design and Research Potential. ZEW Discussion Papers 09-053, ZEW - Zentrum für Europäische Wirtschaftsforschung / Center for European Economic Research.

Fukuyama, Francis (2015). Why is Democracy Performing so Poorly? *Journal of Democracy 26*(1), 11–20.

Galiani, Sebastian, Paul Gertler, and Ernesto Schargrodsky (2008). School Decentralization: Helping the Good Get Better, but Leaving the Poor Behind. *Journal of Public Economics 92*(10), 2106–2120.

Gaviria, Alejandro, Juan Gonzalo Zapata, and Adriana Gonzélez (2002). Petróleo y Región: El caso de Casanare. Nueva Serie Cuadernos de Fedesarrollo 8, Fundación para la Educación Superior y el Desarrollo (Fedesarrollo).

Gebhardt, Frieder (1994). *Wirtschaftsatlas Neue Bundesländer* (1 ed.). Gotha: Perthes.

Geys, Benny, and Kai A. Konrad (2010). Federalism and Optimal Allocation Across Levels of Governance. In *Handbook on Multi-level Governance*. Cheltenham, UK: Edward Elgar Publishing, Inc.

Gibbons, Stephen, and Henry G. Overman (2012, May). Mostly Pointless Spatial Econometrics? *Journal of Regional Science 52*(2), 172–191.

Glaeser, Edward L., Bruce I. Sacerdote, and Jose A. Scheinkman (2003, apr). The Social Multiplier. *Journal of the European Economic Association 1*(2-3), 345–353.

Glitz, Albrecht, and Erik Meyersson (2017, June). Industrial Espionage and Productivity. IZA Discussion Papers 10816, Institute for the Study of Labor (IZA).

Goschler, Constantin, and Marcus Böick (2017). Wahrnehmung und Bewertung der Arbeit der Treuhandanstalt. *Bundesministeriums für Wirtschaft und Energie*.

Graversen, Brian Krogh, and Jan C. van Ours (2008, October). How to Help Unemployed Find Jobs Quickly: Experimental Evidence from a Mandatory Activation Program. *Journal of Public Economics 92*(10-11), 2020–2035.

Gregg, Paul, and Barbara Petrongolo (2005). Stock-Flow Matching and the Performance of the Labor Market. *European Economic Review 49*, 1987–2011.

Gruber, Jonathan (1994). The Incidence of Mandated Maternity Benefits. *The American Economic Review 84*(3), 622–641.

Gupta, Nandini, John C Ham, and Jan Svejnar (2008). Priorities and Sequencing in Privatization: Evidence from Czech Firm Panel Data. *European Economic Review 52*(2), 183–208.

Hainmueller, Jens (2012). Entropy Balancing for Causal Effects: A Multivariate Reweighting Method to Produce Balanced Samples in Observational Studies. *Political Analysis 20*(1), 25–46.

Hainmueller, Jens, Barbara Hofmann, Gerhard Krug, and Katja Wolf (2016). Do Lower Caseloads Improve the Performance of Public Employment Services? New Evidence from German Employment Offices. *The Scandinavian Journal of Economics 118*(4), 941–974.

Halleck Vega, Solmaria, and J. Paul Elhorst (2015). The SLX Model. *Journal of Regional Science 55*(3), 339–363.

Hatzichronoglou, Thomas (1997). Revision of the High-Technology Sector and Product Classification. Technical Report 1997/02, OECD Science, Technology and Industry Working Papers.

Hau, Harald (1998). Privatization Under Political Interference: Evidence from Eastern Germany. *European Economic Review 42*(7), 1177–1201.

Heckman, James J., Hidehiko Ichimura, Jeffrey A. Smith, and Petra E. Todd (1998). Characterizing Selection Bias Using Experimental Data. *Econometrica 66*(5), 1017–1098.

Heckman, James J., Hidehiko Ichimura, and Petra E. Todd (1997). Matching As An Econometric Evaluation Estimator: Evidence from Evaluating a Job Training Programme. *Review of Economic Studies 64*(4), 605–654.

Heckman, James J., Robert J. Lalonde, and Jeffrey A. Smith (1999). The Economics and Econometrics of Active Labor Market Programs. In O. C. Ashenfelter and D. Card (Eds.), *Handbook of Labor Economics*, Volume 3, Part A, Chapter 31, pp. 1865 – 2097. Elsevier.

Heinze, Anja, Alexander Spermann, and Henrik Winterhager (2006). Deregulating Job Placement in Europe: A Microeconometric Evaluation of an Innovative Voucher Scheme in Germany. *Labour Economics 13*(4), 505–517.

Heske, Gerhard (2014). Value Creation, Employment and Investment in East Germany's Industry from 1950 to 2000.

Hines, James R, and Richard H Thaler (1995). Anomalies: The Flypaper Effect. *The Journal of Economic Perspectives 9*(4), 217–226.

Hofmann, Barbara, and Gesine Stephan (2016). Arbeitslose Neuzugänge in den ALG-II-Bezug: Ausgewählte Befunde nach Herkunfts-und Zielbranchen. *IAB, Aktuelle Berichte* (18/2016).

Holzner, Christian, and Sonja Munz (2013). Should Local Public Employment Services Be Merged with Local Social Benefit Administrations? *Journal of Labour Market Research 46*(2), 83–102.

Huber, Martin, Michael Lechner, and Conny Wunsch (2013). The Performance of Estimators Based on the Propensity Score. *Journal of Econometrics 175*(1), 1–21.

Inman, Robert P. (2008, December). The Flypaper Effect. NBER Working Paper 14579, National Bureau of Economic Research.

Isen, Adam (2014, February). Do Local Government Fiscal Spillovers Exist? Evidence from Counties, Municipalities, and School Districts. *Journal of Public Economics 110*, 57–73.

Janik, Florian, and Susanne Kohaut (2012, Apr). Why Don't They Answer? Unit Non-Response in the IAB Establishment Panel. *Quality & Quantity 46*(3), 917–934.

Keen, Michael, and Maurice Marchand (1997, October). Fiscal Competition and the Pattern of Public Spending. *Journal of Public Economics 66*(1), 33–53.

Kluve, Jochen (2010). The Effectiveness of European Active Labor Market Programs. *Labour Economics 17*(6), 904–918.

Kropp, Per, and Barbara Schwengler (2016). Three-Step Method for Delineating Functional Labour Market Regions. *Regional Studies 50*(3), 429–445.

Kühl, Jürgen, Reinhard Schaefer, and Jürgen Wahse (1991). Beschäftigungsperspektiven von Treuhandunternehmen. *Mitteilungen aus der Arbeitsmarkt- und Berufsforschung 3*, 91.

Kühl, Jürgen, Reinhard Schaefer, and Jürgen Wahse (1992a). Beschäftigungsperspektiven von Treuhandunternehmen und Ex-Treuhandfirmen im April 1992. *Mitteilungen aus der Arbeitsmarkt-und Berufsforschung 25*(4), 519–533.

Kühl, Jürgen, Reinhard Schaefer, and Jürgen Wahse (1992b). Beschäftigungsperspektiven von Treuhandunternehmen und Ex-Treuhandfirmen im Oktober 1991. *Mitteilungen aus der Arbeitsmarkt- und Berufsforschung 1*, 32–50.

Laffont, Jean-Jacques, and Jean Tirole (1991). Privatization and Incentives. *Journal of Law, Economics, and Organization 7*(0), 84–105.

Lalive, Rafael, Jan C. van Ours, and Josef Zweimüller (2005). The Effect of Benefit Sanctions on the Duration of Unemployment. *Journal of the European Economic Association 3*(6), 1386–1417.

Lee, David S, and Thomas Lemieux (2010). Regression Discontinuity Designs in Economics. *Journal of Economic Literature 48*(2), 281–355.

Lee, Lung-fei, and Jihai Yu (2010). Estimation of Spatial Autoregressive Panel Data Models with Fixed Effects. *Journal of Econometrics 154*(2), 165–185.

LeSage, James, and Robert Kelley Pace (2009). *Introduction to Spatial Econometrics*. CRC Press.

Lipscomb, Molly, and Ahmed Mushfiq Mobarak (2017). Decentralization and Pollution Spillovers: Evidence from the Re-drawing of County Borders in Brazil. *The Review of Economic Studies 84*(1), 464–502.

Lopez-de Silanes, Florencio, Andrei Shleifer, and Robert Vishny (1997). Privatization in the United States. *RAND Journal of Economics 28*(3), 447–471.

Lülfesmann, Christoph (2007). On the Virtues of Privatization when Government is Benevolent. *Journal of Economic Behavior & Organization 64*(1), 17–34.

Lundin, Martin, and Per Skedinger (2006). Decentralisation of Active Labour Market Policy: The Case of Swedish Local Employment Service Committees. *Journal of Public Economics 90*(4-5), 775 – 798.

Lyytikäinen, Teemu (2012, August). Tax Competition Among Local Governments: Evidence from a Property Tax Reform in Finland. *Journal of Public Economics 96*(7-8), 584–595.

Manski, Charles F (1993). Identification of Endogenous Social Effects: The Reflection Problem. *Review of Economic Studies 60*(3), 531–42.

Martinez-Vazquez, Jorge, Santiago Lago-Peñas, and Agnese Sacchi (2017). The Impact of Fiscal Decentralization: A Survey. *Journal of Economic Surveys*.

Mechtel, Mario, and Niklas Potrafke (2013). Electoral Cycles in Active Labor Market Policies. *Public Choice 156*(1-2), 181–194.

Megginson, William L. (2017). Privatization Trends and Major Deals in 2015 and 2016. In *The PB Report 2015/2016*. The Privatization Barometer.

Moscone, Francesco, Martin Knapp, and Elisa Tosetti (2007, July). Mental Health Expenditure in England: A Spatial Panel Approach. *Journal of Health Economics 26*(4), 842–64.

Musgrave, Richard A. (1959). *The Theory of Public Finance: A Study in Public Economy*. New York: McGraw-Hill.

Nagl, Wolfgang, and Michael Weber (2016). Stuck in a Trap? Long-term Unemployment under Two-tier Unemployment Compensation Schemes. *Ifo Working Paper No. 231*.

Neyapti, Bilin (2010). Fiscal Decentralization and Deficits: International Evidence. *European Journal of Political Economy 26*(2), 155–166.

Oates, Wallace E. (1972). *Fiscal Federalism*. New York: Harcourt Brace Jovanovich.

Otto, James, Craig Andrews, Fred Cawood, M Doggett, G Pietro, F Stermole, J Stermole, and J Tilton (2006). Mining Royalties: A Global Study of Their Impact on Investors, Government, and Civil Society. *Washington, DC: The World Bank*.

Parchet, Raphaël (2014). Are Local Tax Rates Strategic Complements or Strategic Substitutes? IdEP Economic Papers 1407, USI Università della Svizzera italiana.

Perotti, Enrico (1995). Credible Privatization. *American Economic Review 85*(4), 847–59.

Perotti, Roberto (2005). Public Spending on Social Protection in Colombia: Analysis and Proposals. In A. Alesina (Ed.), *Institutional Reforms: The Case of Colombia*, pp. 273–335. MIT Press.

Persson, Torsten, and Guido Tabellini (2008). The Growth Effects of Democracy: Is it Heterogenous and How Can It Be Estimated? In E. Helpman (Ed.), *Institutions and Economic Performance*. Harvard University Press.

Petrongolo, Barbara, and Christopher A. Pissarides (2001). Looking into the Black Box: A Survey of the Matching Function. *Journal of Economic Literature 39*(2), 390–431.

Pissarides, Christopher A. (1979). Job Matchings with State Employment Agencies and Random Search. *The Economic Journal 89*(356), 818–833.

Redding, Stephen J, and Daniel M Sturm (2008). The Costs of Remoteness: Evidence from German Division and Reunification. *The American Economic Review 98*(5), 1766–1797.

Revelli, Federico (2015). Geografiscal Federalism. In G. B. Ehtisham Ahmad (Ed.), *Handbook of Multilevel Finance*, pp. 107–123. Cheltenham, UK: Edward Elgar Publishing.

Roland, Gérard (1993). The Political Economy of Restructuring and Privatization in Eastern Europe. *European Economic Review 37*(2), 533 – 540.

Roland, Gérard (1994). On the Speed and Sequencing of Privatisation and Restructuring. *The Economic Journal 104*(426), 1158–1168.

Roland, Gérard (2002, March). The Political Economy of Transition. *Journal of Economic Perspectives 16*(1), 29–50.

Ruschmeier, René, and Frank Oschmiansky (2010). Organisationsnovelle des SGB II - Die Neuregelungen im Überblick. *Zeitschrift für das Fürsorgewesen 62*(8).

Saavedra, Luz Amparo (2000). A Model of Welfare Competition with Evidence from AFDC. *Journal of Urban Economics 47*(2), 248–279.

Sakshaug, Joseph W., and Martina Huber (2016). An Evaluation of Panel Nonresponse and Linkage Consent Bias in a Survey of Employees in Germany. *Journal of Survey Statistics and Methodology 4*(1), 71–93.

Sánchez, Fabio, Michael Smart, and Juan G Zapata (2004). Intergovernmental Transfers and Municipal Finance in Colombia. International Tax Program Papers 0403, Rotman School of Management, University of Toronto.

Schmidt, Klaus M (1996). Incomplete Contracts and Privatization. *European Economic Review 40*(3-5), 569–579.

Shleifer, Andrei (1985). A Theory of Yardstick Competition. *RAND Journal of Economics 16*(3), 319–327.

Shleifer, Andrei, and Robert W Vishny (1994). Politicians and Firms. *The Quarterly Journal of Economics 109*(4), 995–1025.

Siebert, Horst (1991). German Unification: The Economics of Transition. *Economic Policy 6*(13), 287–340.

Sigman, Hilary (2002, September). International Spillovers and Water Quality in Rivers: Do Countries Free Ride? *American Economic Review 92*(4), 1152–1159.

Smart, Michael (2007). The Incentive Effects of Grants. In R. W. Boadway and A. Shah (Eds.), *Intergovernmental Fiscal Transfers: Principles and Practices*, pp. 203–223. World Bank Publications.

Soto, Victoria Eugenia, Maria Isabel Farfan, and Vincent Lorant (2012). Fiscal Decentralisation and Infant Mortality Rate: The Colombian Case. *Social Science & Medicine 74*(9), 1426–1434.

Szentpèteri, Àdàm, and Àlmos Telegdy (2012). Political Objectives and Privatization Decisions. *Economics of Transition 20*(2), 299–313.

Tiebout, Charles M (1956). A Pure Theory of Local Expenditures. *Journal of Political Economy 64*(5), 416–424.

van den Berg, Gerard J., Arne Uhlendorff, and Joachim Wolff (2014). Sanctions for Young Welfare Recipients. *Nordic Economic Policy Review* (2014:416), 177–208.

van den Berg, Gerard J., Bas van der Klaauw, and Jan C. van Ours (2004). Punitive Sanctions and the Transition Rate from Welfare to Work. *Journal of Labor Economics 22*(1), 211–241.

Viale, Claudia, and Edgardo Cruzado (2012). La distribución de la renta de las industrias extractivas a los gobiernos subnacionales en América Latina. Mimeo, Lima: Revenue Watch Institute.

Villar, Azul del, Lili Liu, Edgardo Mosqueira, Juan Pedro Schmid, and Steven B. Webb (2013). Colombia: Subnational Insolvency Framework. In C. Otaviano and L. Liu (Eds.), *Until Debt Do Us Part*, pp. 179–219. The World Bank.

Vorstand der Bundesagentur für Arbeit (2014, October). *Planung und Steuerung 2015 für die gemeinsamen Einrichtungen der Grundsicherung*. Nürnberg: Internal Document.

Wahse, Jürgen, Vera Dahms, and Reinhard Schaefer (1993). Beschäftigungsperspektiven von Treuhandunternehmen und Ex-Treuhandfirmen Umfrage 10/1992. *Beiträge zur Arbeitsmarkt-und Berufsforschung 1630*(3), 1–283.

Wahse, Jürgen, Vera Dahms, Reinhard Schäfer, and Jürgen Kühl (1996). Beschäftigungsperspektiven von privatisierten Unternehmen. *Mitteilungen aus der Arbeitsmarkt-und Berufsforschung 29*(1), 106–116.

Wapler, Rdiger, Katja Wolf, and Joachim Wolff (2018). Do Active Labour Market Policies for Welfare Recipients in Germany Raise their Regional Outflow into Work?: A Matching Function Approach. IAB Discussion Paper 201808, Institut fr Arbeitsmarkt- und Berufsforschung (IAB), Nürnberg.

Weber, Enzo, Karl Heinz Hausner, and Heidemarie Engelhard (2014). *Gesamtfiskalische Kosten der Arbeitslosigkeit im Jahr 2013 in Deutschland*. Nürnberg: Institut für Arbeitsmarkt- und Berufsforschung. Aktuelle Daten und Indikatoren.

Weingast, Barry R, Kenneth A Shepsle, and Christopher Johnsen (1981). The Political Economy of Benefits and Costs: A Neoclassical Approach to Distributive Politics. *Journal of Political Economy 89*(4), 642–664.

Wildasin, David E (1988). Nash Equilibria in Models of Fiscal Competition. *Journal of Public Economics 35*(2), 229–240.

Wildasin, David E (1991). Income Redistribution in a Common Labor Market. *The American Economic Review 81*(4), 757–774.

World Bank (2017). World Development Indicators. *Available from http://data.worldbank.org/indicator/NY.GDP.PCAP.CD*.

Yu, Jihai, Li-An Zhou, and Guozhong Zhu (2016). Strategic Interaction in Political Competition: Evidence from Spatial Effects Across Chinese Cities. *Regional Science and Urban Economics 57*, 23–37.

Zodrow, George R., and Peter Mieszkowski (1986, May). Pigou, Tiebout, Property Taxation, and the Underprovision of Local Public Goods. *Journal of Urban Economics 19*(3), 356–370.